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Essays on Public Health Insurance and Child Health

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ABSTRACT

ESSAYS ON PUBLIC HEALTH INSURANCE AND CHILD HEALTH

BY

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This dissertation evaluates how publicly subsidized insurance programs impact fertility decisions, maternity services, and the health of children. I focus on two types of subsidized insurance: Medicaid, which is fully subsidized, and non-Medicaid subsidized insurance (NMSI) plans, which are partially subsidized. The first two chapters uses variation from the Affordable Care Act (ACA) to evaluate how eligibility for subsidized insurance affects birth related outcomes. The first chapter examines changes to the birth rate and the second chapter evaluates prenatal care, maternal health behaviors, delivery procedures, and birth outcomes. The third chapter focuses on Medicaid and explores how a shift to managed care affected care utilization for foster children.

The ACA greatly expanded subsidized health insurance opportunities for low-income childless women. This insurance provides better access to prescription-based contraception which could reduce the number of births. At the same time, subsidized insurance creates an income effect and lowers the price of childbirth for women who previously would not have qualified for Medicaid-paid pregnancies. In the first chapter, I use simulated eligibility measures to examine how Medicaid and NMSI policies impacted insurance enrollment and the birthrate for childless women from 2011 through 2016. My results indicate that expanding Medicaid had no significant

effect on the birth rate, but that a 10 percentage point increase in NMSI eligibility increased the birth rate between 0.6% and 1.5%, depending on the age group.

The second chapter focuses on maternity related health care utilization and outcomes. While pregnancy-conditional Medicaid has covered maternity services for low-income pregnant women since the 1980s, new eligibility for subsidized insurance prior to conception via the ACA may improve timely prenatal visits and, in turn, pregnancy behaviors and outcomes. Additionally, women eligible for subsidized Marketplace plans but not pregnancy-conditional Medicaid gained access to subsidized maternity services. Overall, I find no changes in pregnancy and birth outcomes from the Medicaid expansion, but do find evidence of reduced smoking behaviors and increased breastfeeding arising from a higher share of women eligible for NMSI. Additionally, an increase in the share of women eligible for NMSI and not pregnancy-conditional Medicaid reduced the probability of Medicaid paid births.

The third chapter of this dissertation evaluates the impact of transitioning foster children from fee-for-service Medicaid coverage to Medicaid managed care (MMC) on outpatient health care utilization. A recent trend in state Medicaid programs is the transition of vulnerable populations into MMC who were initially carved out of such coverage, such as foster children or those with disabilities. There is very little empirical evidence on the impact of managed care on the health care utilization of foster children because of the recent timing of these transitions as well as challenges associated with finding datasets large enough to contain a sufficient number of foster children for such analysis. Using administrative Medicaid data from Kentucky, a retrospective difference-in-difference analysis compares the outpatient utilization of foster children transitioned to MMC in one region of the state to foster children in the rest of the state who remained in fee-for-service coverage. Results indicate that the transition to MMC led to a 4 percentage point reduction in the probability of having any monthly outpatient utilization, as well as a reduction in outpatient spending. This chapter is coauthored with James Marton, Aaron Yelowitz, and Jeffery Talbert and is published in *Inquiry*. It is reproduced here under the creative commons non-commercial license. When citing, please use the published version: Palmer et al. (2017).

ESSAYS ON PUBLIC HEALTH INSURANCE AND CHILD HEALTH

BY

MAKAYLA PALMER

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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To my father

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LIST OF ABBREVIATIONS

AFDC	Aid to Families with Dependent Children
ACA	Affordable Care Act
ACS	American Community Survey
BMI	Body mass index
CMS	Center for Medicaid and Medicare Services
DD	Difference-in-difference
ESI	Employer sponsored insurance
FFS	Fee-for-service
IOM	Institute of Medicine
IV	Instrumental Variable
KFF	Kaiser Family Foundation
LBW	Low birth weight
MCO	Managed care organization
MMC	Medicaid managed care
NICU	Neonatal intensive care unit
NMSI	Non-Medicaid subsidized insurance
SSI	Supplemental Security Income
TANF	Temporary Assistance for Needy Families
DDD	Triple-Difference
VS	Vital Statistics

INTRODUCTION

This dissertation analyzes how publicly subsidized health insurance impacts outcomes related to populations with substantial medical costs: pregnant women, newborns, and foster children. The first chapter addresses how subsidized insurance affects the birth rate, and the second delves into health care utilization during pregnancy and delivery, maternal health behaviors, and birth outcomes. The third chapter uses a natural experiment to measure how health care utilization changes when foster children transition from fee-for-service (FFS) Medicaid to Medicaid managed care (MMC).

In 2010, when the Affordable Care Act (ACA) was passed, affordable and comprehensive insurance coverage for childless women was primarily limited to employer sponsored insurance (ESI) plans. Medicaid covered low income adults who met both financial and categorical eligibility requirements. Children, pregnant, parents, elderly, the blind, and disabled were categorically eligible for Medicaid, but the majority of childless adults did not meet one of these categories and were therefore ineligible for Medicaid regardless of their incomes. The non-group insurance market frequently excluded maternity services, charged women higher premiums, and could consider pregnancy a pre-existing condition (Garrett et al, 2012). Consequently, women experienced the highest rates of uninsurance during their reproductive years which could have important ramifications for maternity related outcomes such as the birth rate, prenatal care utilization, behaviors during pregnancy, and birth outcomes.

Concern over the lack of insurance for soon-to-be mothers prompted the expansion of Medicaid to low-income pregnant women in the late 1980s. The Institute of Medicine (IOM) found that the primary causes of delayed prenatal care were financial constraints and inadequate insurance. Consequently, the IOM determined expanding Medicaid could help improve birth outcomes and reduce racial disparities in prenatal care and birth outcomes (IOM, 1985). Starting in 1989, all states have been required to cover pregnant women up to 133% of the federal poverty line and many cover pregnant women to even higher income thresholds.

Medicaid coverage for low-income, pregnant women has been an important provider of maternity services, paying for 43% of births to U.S. women of childbearing age in 2010 (author's calculation using VS data). However, several potential gaps remain. First, childless, non-pregnant women who lack insurance coverage face difficulties accessing prescription birth control methods, which are the most effective forms of contraception. Lack of contraception access may cause higher rates of unintended pregnancies and more Medicaid-paid births, which is supported by evidence from programs providing contraception to low-income individuals (Kearny & Levine, 2009; Lindrooth & McCullough, 2007; Sills, 2008; Adams et al., 2015). Additionally, women who are ineligible for Medicaid until they are pregnant may experience delays in prenatal care because of lack of awareness of their eligibility or difficulties in enrolling and making appointments using an unfamiliar insurance plan. Prior to the ACA, women without ESI and incomes above the pregnancy-conditional Medicaid thresholds may have lacked access to contraception services as well. Even more importantly, these women likely lacked coverage for maternity services which would make births incredibly costly medical events.

The first two chapters of this dissertation analyzes how access to subsidized health insurance for childless women affects birth related outcomes which could be influenced by improved access to contraception, cheaper maternity services, and an income effect. The first chapter evaluates how subsidized insurance affects the birth rate. The second chapter examines other birth related outcomes such as the timeliness of prenatal care, maternal behaviors during pregnancy, birth procedures, and birth outcomes.

These two chapters focus on subsidized insurance stratified into two types: Medicaid, which is fully subsidized, and non-Medicaid subsidized insurance (NMSI) plans, which are partially subsidized. The NMSI plans include some state-level plans prior to the ACA and subsidized Marketplace plans. I measure the extent of eligibility for these two categories of subsidized insurance using simulated eligibility measures and I structure the framework of my analysis using a triple-difference approach. The data on births come from all U.S. birth records from 2011-2016 provided by the National Center for Health Statistics. I utilize the American Community Survey to create eligibility measures and estimate insurance related effects.

The first chapter finds that the Medicaid eligibility increased the share of women enrolled in Medicaid while NMSI eligibility increased the share enrolled in non-Medicaid insurance. I find no significant effects on the birth rate resulting from increased Medicaid eligibility, but I find that NMSI eligibility increased the birth rate between 0.6% and 1.5%, depending on the age group. In the second chapter, I find no significant impacts stemming from Medicaid eligibility, but find improvements in birth related outcomes from NMSI eligibility concentrated in areas related to preventative care, which benefited from additional ACA policies. I find NMSI eligibility reduced pre-pregnancy smoking among mothers and produced small, but statistically significant, improvements in the probability a baby is breastfeeding when leaving the hospital. I also find that a higher share of women eligible for NMSI before and during pregnancy lead to reductions in the probability that a birth was paid for by Medicaid.

The third chapter shifts the focus to foster children and changes within Medicaid. Foster children have high rates of chronic physical and mental health needs due to histories of abuse and neglect. Medicaid provides health services for these children and has special policies to help insure they receive adequate care. Medicaid experienced large movements towards MMC plans in the 1990s (Kaye et al, 1999); however, states often kept foster children in FFS plans due to concerns that capitated rates and primary care gatekeepers may prevent foster children from accessing necessary care. While states continue to move foster children to MMC plans, Kentucky mandatorily moved foster children in part of the state into managed care in 1999.

Together with my coauthors, I use administrative data from Kentucky to analyze how the transition from FFS to MMC affected utilization for foster children. We use a difference-in-difference approach to model the Kentucky natural experiment by comparing foster children who remained in FFS with those who were moved into MMC before and after the transition. We find reductions in outpatient utilization at the extensive and intensive margins, however, the magnitudes of these reductions were similar to ones found for non-foster children who previously transitioned to MMC in Kentucky.

CHAPTER 1. EFFECT OF PUBLICLY SUBSIDIZED HEALTH INSURANCE ON THE BIRTH RATE

1.1 Introduction

The rate of uninsurance for women spikes during the late teens and remains high throughout their reproductive years. In 2010, the share of female teenagers 13-18 years old without health insurance was 10.74% while the share for women 19-45 was more than twice that at 22.25% (ACS data). Lack of insurance brings two unique problems for reproductive aged women: avoiding pregnancy and paying for pregnancy.

Low-income women are often employed in jobs without benefits such as health insurance. In addition, premiums for non-group coverage have been prohibitively expensive, charged women more than men, and rarely included maternity care (Garrett et al., 2012). Given the lack of insurance access for low-income, reproductive-aged women, there have been several important public health policy interventions aimed to improve coverage of maternity services over the past several decades. In the late 1980s, Medicaid was expanded to provide coverage for low-income pregnant women to fully cover health expenditures related to their prenatal care, deliveries, and postpartum care. However, granting Medicaid eligibility to low-income pregnant women does not provide assistance for low-income childless women trying to *avoid* becoming pregnant.

Prior to the Affordable Care Act (ACA), childless adults did not fit one of the federally outlined categories for Medicaid eligibility: children, pregnant women, parents, elderly, the blind, and/or disabled. This meant that childless adults were ineligible for Medicaid regardless of how low their income was. Recognizing that low-income women struggled with obtaining contraception, many states obtained family planning waivers to provide contraception to low-income populations in order to prevent Medicaid-paid births. Numerous studies (Adams et al., 2015; Kearney & Levine, 2009; Lindrooth & McCullough, 2007; Sills, 2008) found that these programs were effective at reducing births.

When the ACA passed in 2010, it created opportunities to provide all adults up to 400%FPL with some degree of subsidized health insurance. As written, the initial legislation expanded Medicaid eligibility up to 138%FPL, while those between 139 and 400%FPL could receive sliding scale subsidies for insurance purchased through the new, non-group insurance exchange system called the Marketplace. However, a Supreme Court decision in 2012 made the Medicaid expansion optional for states. In January 2014, subsidized Marketplace plans took effect in all states and the Medicaid expansion took effect in states which chose to expand at that time. In states that did not expand Medicaid, a "coverage gap" was created as childless adults with family income below 100%FPL did not qualify for Medicaid but were also too poor to qualify for Marketplace subsidies.

The purpose of this chapter is to determine whether subsidized insurance eligibility impacts fertility for childless women. Since the subsidized ACA insurance programs provide women with access to free contraception, women gaining coverage may increase the quantity and/or quality of contraception consumed, which may lead to fewer births. On the other hand, subsidized insurance reduces pregnancy-related expenses for women who had previously lacked insurance for maternity services, which may lead to more births. Additionally, subsidized insurance creates an income effect by freeing up family resources that were previously going towards insurance purchases which could improve families' ability to afford children.

This paper uses data based on birth certificate records for the universe of US births from 2011 to 2015 available from the National Center for Health Statistics. I employ a simulated eligibility approach developed by Currie and Gruber (Currie & Gruber, 1996a, 1996b, 2001) where I divide women into demographic groups and then measure the share of a nationally representative sample of low-income women in each group eligible for subsidized insurance in each month and in each state according to that state's laws. This measure of access to publicly subsidized coverage is created using the American Community Survey (ACS) and lagged for gestational age to measure eligibility at the time of conception. I create eligibility measures for two types of insurance: Medicaid and "non-Medicaid subsidized insurance" (NMSI). Medicaid insurance plans are generally a fully-subsidized form of public insurance whereas NMSI plans may only be partially subsidized. Non-Medicaid plans include publicly subsidized health insurance programs for childless adults that existed in some states prior to the ACA expansion and subsidized Marketplace plans.

I find eligibility has a significant impact on the birth rate, but the effects vary by type of insurance. While I find no statistically significant effects on the birth rate as a result of increased Medicaid eligibility, increases in NMSI eligibility increase the birth rate. I stratify the results by age given the significantly different baseline birthrates and trends between age groups. I find a 10 percentage point increase in NMSI eligibility produces a 0.6% increase in the birth rate for women 20-24, 0.7% increase for women 25-34, and 1.46% increase for women 35-45. The differences in effects of these two types of eligibility reflect the fact that women eligible for the Medicaid expansion received a larger reduction in the price of contraception relative to maternity services while women eligible for Marketplace subsidies received a larger relative reduction in the cost of maternity services.

This paper makes several contributions to the literature. The primary contribution is that, as far as I am aware, this is the first paper to evaluate how increases in subsidized insurance stemming from the Medicaid and Marketplace expansion of the ACA impacted the birth rate. To do this, I build upon a triple-difference (DDD) methodology by using simulated eligibility measures for Medicaid and NMSI eligibility. Since the simulated eligibility approach enables me to use more sources of variation than a standard DDD, I can include information on state-level subsidized health insurance plans that existed for childless adult prior to the ACA. I model the effects of changes in Medicaid and NMSI eligibility simultaneously, which is important because eligibility for these programs is interconnected since the size of the expansion of one effects the size of the expansion of the other. Frean et al. (2017) also use simulated eligibility and DDD methods to study how multiple aspects of the ACA affected insurance enrollment, but this appears to be the first paper to use simulated eligibility to look at outcomes beyond enrollment. This paper also adds to numerous studies on how the ACA impacted enrollment (Charles Courtemanche et al., 2017; C. Courtemanche et al., 2016; Frean et al., 2017; Kaestner et al., 2017), though I focus on a special population: reproductive-aged, childless women. Since insurance eligibility should only affect the birth rate through insurance enrollment, I use the analysis of insurance enrollment as a first step and later use a two-stage least squares method to estimate the effect of instrumented enrollment on the birth rate.

1.2 Policy Background

To describe how the ACA overhauled the American health care system, it is necessary to first outline how private and public health insurance operated before the ACA. This section will describe some of the coverage challenges and health assistance policies related to childless adults. It will then explain how the ACA affected this group with a particular focus on insurance issues related to childbearing decisions.

1.2.1 Before the Affordable Care Act

Among childbearing aged women (those 15-45 years old) in 2010, the majority (55.38%) had employer sponsored insurance (ESI) (ACS data). Employers and employees split the cost of this insurance and these plans must provide employees and their spouses with maternity services (Andrews, 2012; Norris, 2017).

Individuals who did not have ESI could purchase insurance through the non-group market; however, coverage was expensive and often excluded important services. A study done on the best-selling non-group plans in 2012 found that 92% were gender-rated, meaning they charged women more than men of the same age for identical coverage. The vast majority of non-group insurance plans did not include maternity coverage. Among the plans studied in 2012, only 12% included maternity coverage, but this varied widely by state and 25 states (of the 47 states and DC studied) had no non-group providing maternity coverage. In some places, women could purchase a "rider" for maternity coverage separately, but they were often prohibitively expensive, had wait periods, and were limited in scope (Garrett et al., 2012). Paying for prenatal care, delivery, and postpartum care out-of-pocket is expensive. Based on data from 2010, the average cost of non-complicated vaginal birth exceeded $8000 and a non-complicated caesarean section was over $14,000 (Childbirth Connection, 2012).

Medicaid coverage was available to people who met both financial and categorical eligibility requirements. Medicaid is a federal and state partnership where each state administers their own program according to certain federal policies and the federal government reimburses states for part of the cost. Under the federal guidelines, eligible populations consisted of children, pregnant women, parents, disabled, or elderly and each eligibility category had separate financial eligibility thresholds. The federal government places some restrictions on the income thresholds for each eligibility categories, but states have the ability to choose their own income thresholds within those bounds, creating inter-state variation. Medicaid typically has neither premiums nor copays so beneficiaries receive fully subsidized health care.

During the 1980s, concerns about income-related birth outcome disparities motivated the expansion of Medicaid to cover low-income pregnant women and their infants (Alexander & Kotelchuck, 2001; Institute of Medicine, 1985; Lu & Halfon, 2003). By 1989, the federal government required states to provide Medicaid to all pregnant women up to 133%FPL and allowed states to expand eligibility for this group to even higher income thresholds (DeLeire et al., 2011). In 2013, the median income threshold among states was 185%FPL and nationwide Medicaid paid for over 43% of births to U.S. residents of childbearing age (Heberlein et al, 2013; Vital Satistics (VS) data). Pregnancy-conditional Medicaid fully subsidizes pregnancy related services including prenatal care, delivery, and 60 days of postpartum care.

In addition to pregnant women, all states are required to offer Medicaid to low-income parents, though the income thresholds are sustainably lower. The federal minimum for the parent eligibility category is the state's 1996 Aid to Families with Dependent Children income threshold. In 2013, the median Medicaid income threshold among states for working parents was 64%FPL, about a third of the median threshold for pregnant women (Heberlein et al., 2013).

Prior to the ACA, standard Medicaid did not cover non-elderly, non-disabled, childless adults regardless of their income because this population was not categorically eligible for coverage. However, states could apply for a waiver from Center for Medicaid and Medicare Services (CMS) to offer Medicaid to childless adults. In order to do this, it needed to be cost-neutral at the federal budget level which was often done by reallocating disproportionate share hospital payments. Other states created their own programs to offer insurance coverage to low-income adults that were state-funded plans which received no reimbursement from the federal government. In 2013, 25 states had some type of publicly subsidized insurance program for low-income childless adults; however nine of those states' plans had reached the enrollment caps and two more states' plans were restricted to employees of small companies (Heberlein et al., 2013). In the appendix, Table A.1 contains a list of plans that were available to childless adults based solely on their income, meaning it excludes waitlisted programs and those that operate through subsidies for participating employers. Insurance plans that I classified as "Medicaid" based on information from the Kaiser Family Foundation (KFF) are denoted with an asterisk (KFF, 2017a).

New York's "Family Health Plus" plan is an example of a waiver-funded Medicaid program. It offered insurance to adults up to 100%FPL with modest copays but no premiums. Alternatively, Pennsylvania's "adultBasic" plan was state-funded, covered adults up to 200%FPL before it reached capacity, and had copays as well as premiums (KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010)(KFF, 2010). Although it is not feasible to model the intricacies of what the plans' premiums and copays were or the type of benefits covered, this paper does take into account the income thresholds of these pre-ACA subsidized insurance plans for low-income, childless adults.

1.2.2 Affordable Care Act

The ACA overhauled the American health care system with a goal of reducing the number of Americans without comprehensive insurance. It reformed the non-group health insurance market so people could not be denied coverage or charged more for preexisting conditions or gender. To do this without drastically raising the prices of insurance, the ACA needed to discourage healthy people from self-selecting out and driving up the average health costs in the insurance pools. Thus, the ACA mandated that everyone have insurance and created penalties for those without. In order to make the mandatory insurance affordable, the ACA drastically expanded eligibility for publicly subsidized health insurance.

The ACA created a new, non-group health insurance system called the Marketplace. Individuals between 100 and 400%FPL who lacked access to other insurance plans with minimum essential coverage could receive subsidies for Marketplace plans ((26 U.S.C. § 36b (c)(1)(A); (26 U.S.C. § 36b (c)(2)(B)(i))). These subsidies decrease with an individual's income, and those ineligible for subsidies could purchase Marketplace plans at full price. Those with incomes at or below 138%FPL, were eligible for fully subsidized insurance through Medicaid. This move away from the historic categorical eligibility requirements meant that low-income childless adults could now be Medicaid eligible.

However, in an unanticipated decision, the Supreme Court ruled in 2012 that requiring states to expand Medicaid was unconstitutional. States were then given the option to expand Medicaid or not. Five states and the District of Columbia chose to implement an early Medicaid expansion before January 1, 2014, the original start date outlined in the ACA. On January 1, 2014, Medicaid expansion became effective in another 19 states. Between February 2014 and February 2016, six more states expansions went into effect. As of February 2016, 30 states and the District of Columbia had expanded their Medicaid program and 20 have not (KFF, 2017d).

The Supreme Court's ruling did not affect the tax subsidies for Marketplace plans so these subsidized plans became effective in all states January 1, 2014.[1] Plans purchased on the Marketplace can qualify for two types of subsidies: premium tax credits and cost sharing reduction (CSR) subsidies. The premium tax credits cap the share of income that people pay on premiums.

[1] These subsidies were challenged in a 2015 Supreme Court case but were upheld as constitutional.

The CSR subsidies lower a plan's deductible, copays, and out of pocket maximum. The value of these subsidies varies based on an individual's income, what plans are available in their area, what plan they choose, and how much care they use (Norris, 2018).

As a consequence of the Medicaid expansion being optional, subsidized insurance opportunities available vary by state. In non-Medicaid expansion states, Medicaid maintained the system of categorical and financial eligibility requirements under which childless adults remain ineligible. Childless adults between 100 and 400%FPL became eligible for Marketplace subsidies. In expansion states, adults up to 138%FPL became eligible for Medicaid and adults between 139 and 400%FPL became eligible for Marketplace subsidies. Since Medicaid meets the minimum essential coverage requirements (26 U.S.C. § 5000A (f)(1)(A)(ii)), people eligible for Medicaid are not eligible for Marketplace subsidies. This explains why the income threshold lower-bound for Marketplace subsidies in expansion states is 138%FPL instead of 100%FPL. In expansions states, all adults below 400%FPL are eligible for some form of subsidized health insurance, but in non-expansion states those below 100%FPL fall into the coverage gap. Too poor to qualify for Marketplace subsidies, but also ineligible for Medicaid unless they met pre-ACA Medicaid eligibility requirements, the ACA did expand any form of subsidies for this group. In 2016, an estimated 2.6 million adults were in this situation (Garfield & Damico, 2016).

In addition to the Marketplace subsidies, other aspects of ACA's reforms to the non-group improved the situation for women. Under the ACA, gender-rating is no longer allowed. Maternity care and childbirth are considered essential health benefits and must be included in all qualified health plans. Additionally, some pre and postnatal care services are considered preventive care such as gestational diabetes screening, folic acid supplements, and breastfeeding counseling. These services must be provided without copayments or coinsurance even before the yearly deductible is met (Healthcare.gov, 2017).

Beyond the Medicaid and Marketplace expansion, there were some additional aspects of the ACA which are relevant for childbearing decisions. The first is the requirement that all new, private health insurance plans provide at least one of all the 18 FDA-approved categories of female contraception without a copay (Sobel et al., 2016).[2] This provision took effect August, 2012 but the changes were made to insurance plans when the contracts were renewed (Carlin et al., 2016; Sobel et al., 2016).

While the contraception mandate is related to the larger question of how access to affordable and effective contraception impacts the birth rate, this paper focuses on the effects of eligibility for subsidized insurance rather than specifics of those plans. Medicaid does not have copays, so people eligible for Medicaid have always had no cost sharing on contraception. Marketplace plans where not established until after the contraception mandate took place and therefore have also always had no-cost sharing for contraception.

Another provision allows young adults to stay on their parents' ESI plans until they turned 26. This provision took effect September 2010 but some insurers voluntarily agreed to allow this earlier (Akosa Antwi et al., 2013). This increased the number of young adults who had health insurance by around 5 percentage points depending on the study (Akosa Antwi et al., 2013; Barbaresco et al., 2015; Sommers et al., 2012). To avoid changes in coverage and childbearing

[2] There are exceptions for houses of worship employers and accommodations for religiously affiliated non-profits and closely-held corporations (Sobel et al., 2016; Sobel & Salganicoff, 2016). The legal battles over whether this mandate violates freedom of religion has led to two Supreme Court cases: Burwell v. Hobby Lobby and *Zubik v. Burwell.* The Trump administration recently announced that they would be further expanding exceptions for employers with sincerely held religious beliefs against some or all forms of contraception (Pear et al., 2017).

decisions due to the dependent coverage mandate, I start the analysis with data on 2011 conceptions.

1.3 How the ACA May Impact Births

Gaining access to insurance for women of reproductive health could affect the birth rate through several different channels, and the overall direction will depend on which of these channels dominates. This section will outline some of those channels and review related literature to inform our understanding of the various channel's effects from other contexts.

Subsidized insurance could reduce the birth rate by improving access and affordability of the most effective forms of birth control. Improved contraception could lead to fewer unintended pregnancies and births. Unintended pregnancies are classified as "unwanted," meaning the mother did not want to become pregnant any time in the future, or "mistimed" meaning the mother wanted to become pregnant later than she did (Alan Guttmacher Institute, 2016; Kost, 2015). Focusing on the short term effects of expanding insurance is beneficial because it has the potential to detect reductions in both unwanted and mistimed births. As pregnancies that would have been mistimed are delayed to their intended times, there could be some reversion.

Of course, not all unintended pregnancies result in births. Abortions could mitigate the effects of eligibility on births. Every pregnancy results in one of three outcomes: a birth, an abortion, or a loss. Holding losses constant, reductions in unintended pregnancies could manifest themselves as fewer births or fewer abortions. If all unintended pregnancies resulted in abortions, then improved access to contraception would reduce the number of abortions, but not the number of births. In actuality, about 43% of unintended pregnancies result in abortions, which would absorb a sizeable share of the effect of contraception on births (L. B. Finer & Zolna, 2011). Studying how the ACA expansion affects abortions is an important part of understanding how subsidized insurance affects unintended pregnancy rates, but given the longer delay on abortion data, such an analysis now would be premature.

The effects of improved access to contraception are likely larger for women gaining coverage through the Medicaid expansion and younger women. In 2010, the unintended pregnancy rate was five times higher for women below the federal poverty line than those at 200%FPl and higher (L. B. Finer & Zolna, 2011). Since the former group is eligible for Medicaid (in states which expanded) and the latter is eligible for NMSI, there is more room to reduce unintended pregnancies among the Medicaid eligible group. Additionally, while the share of pregnancies that are unintended is highest among teens, the rate of unintended pregnancies (how many unintended pregnancies there are per 1,000 women) is highest among 20-24 year olds (L. B. Finer & Zolna, 2011). Consequently, the effects of improved access to contraception (via an expansion in coverage) may have the largest impact on births for women 20-24.

Subsidized insurance also subsidizes maternity services, which could financially enable more women to have children. Prior to the ACA, low-income women could use Medicaid to pay for prenatal, childbirth, and postnatal care. For women with incomes low enough to qualify for Medicaid when pregnant, childbirth was essentially free. However, for women above the income thresholds for Medicaid for pregnancy and without group health insurance, the costs were substantial. Women under 138%FPL who qualify for Medicaid under the Medicaid expansion would have qualified for Medicaid for pregnancy prior to the expansion in all states. For these women, the expansion did not change the cost of childbirth. However, for women above their state's Medicaid threshold for pregnant women and below 400%FPL, insurance coverage greatly reduced the cost of childbirth. The upward pressure on the birth rate which stems from cheaper maternity services should be larger for NMSI plans than for Medicaid. Given that older women are more likely to have pregnancy complications which makes giving birth more expensive, subsidized maternity services likely larger for older mothers. Older women may also have felt more pent-up demand for a child or feel more urgency to have a child which could make this group more responsive to subsidized maternity services.

At a more general level, both Medicaid and NMSI coverage contain an income effect. By subsidizing the cost of health insurance prior to conception and foreseeably afterwards, women have more money which can be spend on children, which could manifest through either a higher quantity or quality of children. There is some evidence that children are “normal goods” (G. S. Becker, 1960), implying more income from subsidizing insurance would lead to more children; but the elasticity of quality is likely higher than the elasticity of quantity for children which suggests that more of the income would go to increasing quality (G. S. Becker, 1960; G. S. Becker & Lewis, 1973). While subsidies represent a new source of income for women who insurance without the ACA expasion and Medicaid enrollees, the income effect is less clear for newly insured women who take up NMSI plans. If these women purchased insurance because of the ACA, their income less health expenditures may fall.

Insurance could also increase coverage for infertility treatments or abortions. However, the majority of Medicaid and Marketplace plans do not cover infertility treatment (Devine et al., 2014; Schwalberg et al., 2001). Federal funding cannot be used to fund abortions except for pregnancies that endanger the mother’s life or are the result of rape or incest, and that applies to both Medicaid and Marketplace plans. Medicaid covers abortions in 17 states using state funds (Guttmacher Institute, 2018). Since state-funded abortions were part of Medicaid in those states for pregnant women prior to the ACA, expanding Medicaid eligibility for childless adults does not expand Medicaid coverage of abortions. Marketplace plans offering abortion coverage face some administrative complexity because the federally funded premium and cost sharing reduction subsidies cannot help fund abortions. Plans must charge an additional premium to cover abortion coverage (typically $1 per enrollee per month) and are subject to additional reporting and audit requirements. In 2015, 25 states had laws prohibiting Marketplace plans from covering abortion and another 6 states had no laws against abortion coverage but no Marketplace plans chose to offer it. An estimated 62% of uninsured women eligible for subsidies could not choose a Marketplace plan with abortion coverage (Salganicoff et al., 2016).

1.3.1 Insurance and cheaper contraception

Several different types of studies have shown that improving access and the cost of contraception increases utilization and reduces births. This has shown to be true for Medicaid family planning waivers in low-income populations, in private populations from state contraception mandates, and also from the ACA's contraception mandate.

States recognized that the lack of access to contraception for low-income populations resulted in a number of unintended births which were likely paid for by Medicaid. Given the low price of contraception relative to the cost of maternity services, contraception is highly cost effective (Mavranezouli, 2009). States reasoned they could save money by using Medicaid funds to pay for contraception for low-income adults if the program averted enough births among women who would become Medicaid eligible if pregnant. In order to use Medicaid funds in this non-standard way, a state needed a Section 1115 waiver approved by the CMS which required the program to be cost neutral at the federal level.

Several evaluations of family planning waivers found they are effective at reducing births (Kearney & Levine, 2009; Lindrooth & McCullough, 2007; Sills, 2008). One multi-state evaluation found these waivers reduced the number of non-teen births by 2% and teen births by

4% (Kearney & Levine, 2009). Specifically, these programs seemed to reduce births from unintended pregnancies. A study of Illinois and New York's waivers found a reduction in the share of Medicaid-paid births that stemmed from unwanted pregnancies by almost 5 percentage points, and a waiver in Oregon reduced the share which were mistimed pregnancies by almost 9 percentage points (Adams et al., 2015). The reductions in births have made the waivers consistently cost neutral or even cost savings at the federal level. With the cost effectiveness of these programs established, the ACA included a provision for states to provide family planning services in this way using a state plan amendment which does not have to be regularly renewed like a waiver (Ranji et al., 2016). As of January 2014, thirty-one states had family planning expansions programs funded using waivers, SPAs, or state-only funds.

Prior to the ACA's federal contraception mandate, 30 states had their own contraception mandates for employer-sponsored health insurance (Mulligan, 2015). The scope of these mandates was limited because they did not apply to employers who self-insure since they are under federal, rather than state, jurisdiction. Furthermore, while they mandated coverage of contraception, cost sharing could still apply which differs from the no-cost sharing feature of the ACA mandate. States contraception mandates were associated with more contraception use, with larger increases in use among privately insured women (Atkins & Bradford, 2014; Magnusson et al., 2012; Mulligan, 2015) and more consistent use among privately insured women (Magnusson et al., 2012). Mulligan (2015) and Dills and Grecu (2017) find an insignificant decrease in the birth rate, but the effect is likely attenuated by the fact that they used all women rather than privately insured women. Johnston and Adams (2017) found the mandates were associated with fewer mistimed births for privately insured women starting in the second year of implementation.

The ACA's contraception mandate that insurers provide at least one form of contraception without a copay in each of the 18 FDA-approved categories of female contraception went further than state contraception mandates. Studies shows that it significantly reduced the price of contraception, increased contraception use, and reduced births. As a result of the mandate, the median cost of contraception fell to $0 (N. V. Becker & Polsky, 2015; Lawrence B Finer et al., 2014; Pace et al., 2016a, 2016b). One study found the ACA contraception mandate led to a 2.3 percentage point increase in the use of any prescription contraception, and LARC methods in particular, among women with ESI (Carlin et al., 2016). However, another study found no significant increase in LARC use after 2013 (Pace et al., 2016b). For oral contraception, lower copays were associated with less discontinuation and nonadherence (Pace et al., 2016a). Along with an increase in prescription contraception, the ACA contraception mandate led to a decrease in condom sales and a decrease in the birth rate (Willage, 2018).

The literature on family planning waivers and contraception coverage mandates indicates that contraception use increases following price reductions and improved access, and that this in turn can reduce births, particularly unintended ones. This suggests that the ACA may reduce the birth rate and births resulting from unintended pregnancies.

1.3.2 Insurance and cheaper maternity services

Over time there have been several insurance expansions which provided cheaper maternity services, and the effects of these programs on the birth rate have been mixed.

Two studies have found sizeable effects of private insurance on birth decisions. The RAND Health Insurance Experiment randomized enrollment into different health insurance plans and then measured various health outcomes and utilization measures, including births. Among women with no cost sharing for insurance the number of births was 29% higher than for women with cost sharing, who still had a maximum out of pocket cost of $1,000 (Leibowitz, 1990). One interpretation of these results is lowering the cost of maternity services can produce a sizeable increase in births. However, given the temporary nature of this price reduction which for most families was between three and five years, it is also possible that women who planned of giving birth later shifted these births earlier to take advantage of the free medical care. Whether reducing the cost of maternity services influences the timing of births more than the overall number, free maternity services did have a large effect on births in the short term. Another study of an HMO plan in California during the late 1970s found new enrollees had twice the number of births than enrollees who had been enrolled longer than a year. This provides some evidence that women who are pregnant or planning on becoming pregnant may choose insurance which provides maternity benefits (Hudes et al., 1980).

States rolled out Medicaid coverage for low-income pregnant women in the 1980s, dramatically reducing the cost of maternity services by making them essentially free. Unlike the Health Insurance Experiment's findings for no copay maternity services, the Medicaid expansion did not lead to a major increase in the birth rate. The several studies examining this expansion found no robust impact on the birth rate (DeLeire et al., 2011; Joyce et al., 1998; Zavodny & Bitler, 2010). DeLeire et al. (2011) and Joyce et al. (1998) find some indication of a positive effect for white women, whereas DeLeire et al. (2011) initially find larger increases among blacks. This difference could be due to women not being aware that they are eligible for Medicaid conditional on pregnancy or because low-income women had less desire to become pregnant.

The Massachusetts health insurance expansion in 2006, after which the ACA was modeled, increased the share of people in Massachusetts with health insurance. Apostolova-Mihaylova and

Yelowitz (2018) find this increased the birthrate among married women by 1% but reduced the birth rate for unmarried women around 8%.

Several studies found that the ACA's dependent coverage mandate reduced the birth rate by between 5 and 11% (Abramowitz, 2017; Heim et al., 2017; Ma, 2015). It is important to mention, though overlooked in these papers, that while employers are legally required to provide maternity services to employees and their spouses, maternity services have not been mandatory to cover for employee's dependents (Andrews, 2012; Insure, 2016; Norris, 2017; Palanker, 2015; Santos, 2015). It is estimated that the majority of ESI plans do not provide these services for dependents (Andrews, 2012). Guidance on the ACA issued in May 2015 clarified that preventative care including prenatal care visits must be provided to dependents without cost sharing, though other maternity services such as delivery expenses need not be covered (Palanker, 2015; Santos, 2015). Before this clarification was made, there were incidences where even prenatal care claims were denied (Andrews, 2012; Insure, 2016). Given that maternity services are essential health benefits and must be covered by Marketplace, dependents are now one of the only groups whose insurance plans do not necessarily cover maternity services. The fact that the dependent coverage expansion likely led to larger numbers of women gaining subsidized contraception than subsidized maternity services helps explain the decrease in the birth rate that was found by studies on the young adult provision.

1.3.3 ACA and insurance take-up

Eligibility for subsidized insurance should effect the birth rate through increased insurance enrollment rather than directly; therefore, documenting that this eligibility expansion resulted in an increase in insurance coverage is necessary. Several papers using a variety of data sources have demonstrated that the ACA expansions of Medicaid and the Marketplace plans did produce significant increases in insurance enrollment, with largest increases among samples of nonelderly adults (Charles Courtemanche et al., 2017; C. Courtemanche et al., 2016; Frean et al., 2017; Kaestner et al., 2017; Long et al., 2014; Smith & Medalia, 2015; Wherry & Miller, 2016). The larger increase among nonelderly adults is expected given that elderly populations have been eligible for Medicare and children have had generous Medicaid and CHIP eligibility, which makes the ACA subsidized insurance expansions particularly relevant for reproductive aged women.

1.4 Data

1.4.1 American Community Survey and Eligibility

The first key source of data for this paper is the American Community Survey (ACS) from the Census Bureau. I use the one-year sample of the Public Use Microdata Sample data from 2010 to 2016 for the main results. This a large survey that samples approximately 1% of the US population each year and participants are legally obligated to participate. It contains demographic information as well as information on insurance and income, allowing for estimates of eligibility and how it affected enrollment into various types of health insurance plans.

The ACS asks respondents if they are insured through Medicaid, Medicare, directly purchased health insurance, employer-sponsored health insurance (ESI), Tricare, VA, or Indian Health Service. I aggregate these categories into two insurance groups: Medicaid and non-Medicaid insurance. One reason for this is the concern whether individuals can accurately differentiate their type of insurance among these finer categories (Abraham et al., 2013; Charles Courtemanche et al., 2017). Another reason is that the non-Medicaid forms of health insurance are relatively good substitutes for each other. Insurers who sell plans on the non-group market often also have ESI plans, such as Anthem or Aetna. Medicaid differs from these other forms of insurance in that it is nearly free but may have a restrictive network. Finally, having the two categories of Medicaid and non-Medicaid insurance partitions all insurance coverage in a way that aligns with the Medicaid and NMSI eligibility measures.

In order to construct my policy eligibility measures, I collected detailed information on eligibility for several health policies. I use information on childless adult plan eligibility prior to the ACA, pregnancy-conditional Medicaid thresholds, and Medicaid and Marketplace eligibility through the ACA. For Medicaid and non-Medicaid subsidized plans for childless adults prior to January 2014, I used policy information from Kaiser Family Foundation (KFF) as a starting point (Dorn et al., 2004; 2017; 2010, 2017a). I classify some of these childless adult plans as Medicaid based on KFF's classification (KFF, 2017a). When there were changes in the income thresholds between years, I found the dates those changes were effective through documentation on CMS or state websites.

While the structure of Medicaid and CHIP plans are fairly consistent across states, childless adult plans varied greatly. Some of these programs had caps on the number of childless adults who could be enrolled. When a program reached capacity, I no longer counted it as an eligibility option. Between 2010 and 2013, Arizona and Colorado had plans that reached capacity and other states, such as Pennsylvania, were always at capacity. Information on the rate of unintended pregnancies for states in 2010 come from Kost (2015).

The data on which states expanded Medicaid and when comes from KFF (KFF, 2017b). The ACA provided states with the opportunity to expand Medicaid early, and five states plus the District of Columbia did this in various ways. Connecticut, DC, and Minnesota used the early expansion to receive federal reimbursement for their already existing childless adult plans so there were not large increases in enrollment in these states. Washington's childless adult plan was at capacity and did not open enrollment to any new beneficiaries with its early expansion waiver.

New Jersey's early expansion did create a new program, but it only covered childless adults with income at or below 24%FPL.[3] These early expansions highlight the importance of going beyond the expansion dates to include additional information such as income thresholds and enrollment caps. On January 1, 2014 Medicaid coverage for childless adults became effective in 19 more states. Michigan and New Hampshire expanded Medicaid later in 2014; Pennsylvania, Indiana, and Alaska expanded in early 2015; and Montana expanded in early 2016. There is no post expansion data available in the 2016 birth records for expansions in Louisiana and Maine since these did not occur before April 2016.[4]

Before 2013, states allowed income disregards for certain expenditures which made the income thresholds on their Medicaid, CHIP, and waiver programs effectively higher than reported. Two states could have the same income threshold, but one state could be more generous by allowing for more income disregards, making it harder to compare the income thresholds across states. To simplify this, the ACA removed income disregards for certain expenditures and moved to a more standard system of Modified Adjusted Gross Income (MAGI) generally effective January 1, 2014. In order to keep coverage levels constant during this change, the average amount of disregards for people around the threshold in each category in each state were added to the income eligibility threshold. Thus if the threshold had been 133%FPL and the average income disregard was 6%FPL, the new threshold would be 139%FPL, though this would not be an eligibility expansion in any practical sense. From the CMS website, I took state's fillings about their MAGI adjustments and added the average income disregard retroactively so 2014 would not appear to have an increase in eligibility driven by the MAGI change. In addition to the MAGI

[3] California's early expansion roll out was done at the county level, and because of this finer level of geographic variation I excluded California.

[4] The Maine expansion date has not yet been determined.

Table 1.1 Summary Statistics

	Pre 2011-2013	Post 2014-2016	Pre vs Post P value
Medicaid Eligibility			
Aged 20-24	0.041	0.161	0.000
25-34	0.027	0.098	0.000
35-45	0.026	0.106	0.000
NMSI Eligibility			
Aged 20-24	0.038	0.476	0.000
25-34	0.024	0.431	0.000
35-45	0.024	0.411	0.000
Share of Women with any Insurance			
Aged 20-24	0.775	0.858	0.000
25-34	0.763	0.846	0.000
35-45	0.762	0.836	0.000
Share of Women Enrolled in Medicaid			
Aged 20-24	0.092	0.123	0.000
25-34	0.081	0.117	0.000
35-45	0.085	0.125	0.000
Share of Women Enrolled in non-Medicaid Insurance			
Aged 20-24	0.683	0.735	0.000
25-34	0.681	0.729	0.000
35-45	0.677	0.711	0.000
Share of Women Enrolled in ESI			
Aged 20-24	0.558	0.591	0.000
25-34	0.582	0.599	0.074
35-45	0.573	0.576	0.749
Annual Birth Rate (Births per 1,000 Women)			
Aged 20-24	50.699	47.109	0.072
25-34	66.790	64.771	0.610
35-45	15.872	17.125	0.260

Source: Insurance eligibility comes from ACS and information on state level policies. Insurance enrollment data comes from the ACS. The annual birth rates were calculated using the US natality records, the ACS, and the Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data.

adjustment, a standard 5%FPL income disregard was added which does represent an eligibility expansion. For this reason the 5%FPL disregard was not retroactively added to my policy eligibility measures (NH Department of Health & Human Services, 2015).

Table 1.1 presents the mean values for insurance and eligibility measures from the ACS data pre and post 2014 for the sample by age groups. This table shows the increase in the insurance eligibility measures for Medicaid and NMSI plans and the increase Medicaid and non-Medicaid insurance enrollment. Following the trends for each age group, the birth rate fell for younger woman and increased for older women.

1.4.2 Natality Records for Birth Data

Information on births comes from the vital statistics (VS) data from the National Center for Health Statistics with restricted-access geographic identifiers. This dataset contains individual level information taken from birth certificates on all live births in the United States. Having the complete universe of births allows for accurate estimations of the birth rate and a large enough sample to focus on small population groups. The dataset contains demographic, health care utilization, and health outcome data for each baby and their mother. Specifically, the dataset contains the mother's race, age, education, marital status, and state of residency. However, it does not include mother's insurance at the time of conception or any income information. I use the birth records which provide information on births with conceptions from 2011 to 2016. The data contains 46 states and the District of Columbia. I drop Connecticut and Rhode Island due to missing mother educational data.[5] I also exclude Massachusetts and California. Massachusetts had

[5] Some states are missing education information for their birth records. During the study period, most states missing this data appear to start collecting it and their share of records missing education data diminishes rapidly. Because the missing data seems to be driven by collection policies rather than individual mothers not reporting their education, missing education information should be approximately randomly distributed across births. However, I need this information to classify women and consequently I drop state-months where more than 5% of the births are missing education data. When the share missing is less than 5%, I scale the number of births up by the share missing.

a major health insurance overhaul in 2006, making it substantially different from other states. California expanded Medicaid early using a county-level roll out, which does not align with the state-level observational approach used in this paper.

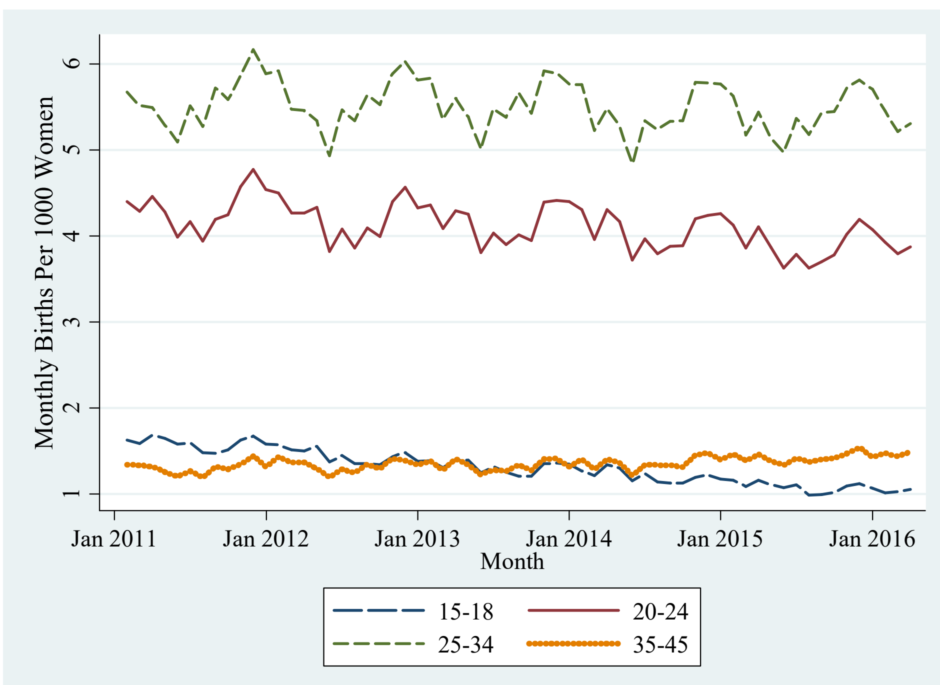
Given the different eligibility requirements for parents and childless adults, I focus on childless women by restricting my sample to births which are a mother's first live birth.[6] Studies on the birth rate typically use reproductive-aged women defined as ages 15 through 45. My study will focus on adult women 20-45 and use teens 15-18 in a robustness check. Teens up to age 18 had fairly generous eligibility for Medicaid and CHIP plans prior to the ACA and did not experience substantial changes in this eligibility. I exclude mothers who are 19 when they give birth because it is unclear whether they were 18 or 19 at the time of conception, which has a large impact on their eligibility for subsidized coverage. Since Marketplace plans and their subsidies are available to women who are citizens or legally present, I use only women who are U.S. residents in the sample (Siskin & Lunder, 2016).

Calculating birth rates at the demographic group-level requires several steps. First, I calculate how many births there were at the group, state, month level in the natality data. Since the natality records are the complete universe of US births, no births in a group-state-month represents a true zero rather than unobserved information. I therefore set the birth rate equal to zero in those cases which provides me with a perfectly balanced panel.

To create a birth rate, I need to divide the number of births by the number of women in that state and demographic group. Census Bureau data gives yearly, state-level estimates of populations by race, age, and sex.[7] To break the population down further by education and marital status, I

[6] This is an imperfect proxy for whether the woman is childless because it counts women who are mothers to non-biological children as childless and counts mothers whose previous children died or were put up for adoption as having children. It is, however, the closest identification of mothers possible given the data.
[7] Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data.

generate shares of women nationally in each age and race group that fall into various education and marital sub-groups. For example, the number of white women, 20-24, with high school degrees, married, and childless in Alaska is the number of white, 20-24 year old women in Alaska multiplied by the share of white, 20-24 year old women who are have high school degrees, are married, and are childless. Using the Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data and combining ACS data for the allocations of education and race among those larger groups gives much more reasonable population estimates than using the ACS data alone. A similar procedure for calculating the birth rate denominators was done by Zavodny and Bitler (2010) in their paper on how Medicaid expansions for pregnant women affected fertility. Finally, I multiply by 1,000 since the birth rate is the number of births per 1,000 women.



Source: US natality records, the ACS, and the Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data.

Figure 1.1 Trends in the Monthly Birth Rate by Age Group

Births are cyclical over the course of the year, with most births occurring in the third quarter. In order to capture the most variation in births as well as best match births to the policy environment at the time of conception, I conduct initial analysis on monthly birth rates, which is the number of births per a 1,000 women in a demographic group in a given month. Figure 1.1 shows that trends in the monthly birth rate over time and by age group. There are substantial differences in the mean birth rate and its trends by age group due to differences in latent fertility, unintended pregnancies, and preferences on when to have children. The birth rate is falling over time for teens and women 20-24, is fairly stable for women 25-34, and is increasing for women 35-45. Given these differences fully stratify the analysis by age groups.

1.5 Methods

The preferred specification in this paper is based upon a DDD framework, but the variables of interest are based on the simulated eligibility approach developed by Currie and Gruber (1996a, 1996b, 2001). My methodology can be seen as operating at the intersection of a DDD and a simulated eligibility approach. Frean et al. (2017) also utilize both DDD and simulated eligibility measures in their paper on the ACA's impact on insurance coverage.

In this section, I start by describing the DDD approach and then explain how this can be improved upon by using simulated eligibility variables. I then take the analysis a step further and use a two-stage least squares (2SLS) methodology to estimate the effects of enrollment on the birth rate. I also describe another classification of eligibility measures which take into consideration pregnancy-conditional Medicaid eligibility. Finally, I lay out several specifications to test the validity of the approach.

1.5.1 Triple-Difference

The different state plans prior to 2014 and the differential roll out of the Affordable Care Act provide variation which can be used to obtain causal estimates of how access to subsidized health insurance affects the birth rate. Viewing the Medicaid expansion as a natural experiment, states which did not expand Medicaid could serve as control states to provide an approximate counterfactual case of what would have occurred in expansion states without the expansion. This could be modeled using a difference-in-difference (DD) approach which compares the differences between expansion and non-expansion states before and after the expansion. Figure A.3 and Figure A.4 show the similarities in the insurance rates and the birth rates for states based on when then expanded Medicaid.

Taken a step further, this expansion can be analyzed using a DDD methodology which incorporates the idea that the effects of Medicaid expansion should be largest where there is a larger share of women below 138%FPL who would become eligible. The DDD approach relies on the weaker assumptions that difference in trends and differential changes in trends between expansion and non-expansions states must not be correlated with the share below 138%FPL in the absence of the ACA, conditional on other controls.

Looking at annual, demographic group level data, I can use the following DDD model to analyze the Medicaid expansion:

$$\begin{aligned} outcome_{gsy} = {} & \beta_0 + \beta_1 PostXExpander_{gsy} + \beta_2 PostXBelow138_{gsy} \\ & + \beta_3 PostXExpanderXBelow138_{gsy} + \beta_4 UR_{sy} + \delta_{gs} + \delta_y + \varepsilon_{gsy} \end{aligned} \qquad (1)$$

The dependent variable $outcome_{gsy}$ represents an outcome variable for women in group g in state s in year *y*. The indicator $Post_y$ for data January 2014 and after does not appear in the model because is perfectly collinear with the year fixed effects δ_y. The indicator for expansion states $Expander_s$ and the variable for the share of a demographic group in a state below 138%FPL ($Below138_{gs}$) are handled by the group-by-state fixed effects δ_{gs}. This leaves the two-way interactions, $PostXExpander_{gsy}$ and $PostXBelow138_{gsy}$, as controls. I also include the annual state unemployment rate UR_{sy} as a control. The effect of the Medicaid expansion would then be $\beta_3 Below138_{gs}$. I use heteroscedasticity-robust standard errors clustered at the state level and observations are weighted by the population of the group-by-state. Weighting makes the group level analysis nationally representative and also places less importance on smaller groups where eligibility and insurance shares are measured with less precision.

The large variation in the birth rate by age indicates that fertility decisions are quite different for younger versus older women. Consequently, eligibility may affect the fertility decisions of women differently based on their age. In order to determine the effect of subsidized insurance and best fit the regressions for each age group, I run each specification separately for women 20-24, 25-34, and 35-45.

Equation (1) only reflects the Medicaid expansion part of the ACA. To incorporate the Marketplace expansion, I run an additional specification where I add $PostX139to400_{gsy}$. I choose the share between 139 and 400%FPL since this is the income range for Marketplace subsidies in states which expanded. Since the Marketplace expansion was national, I do not interact this with whether a state expanded. Results from these two DDD approaches, as well as a simple DD regression, are presented in Table 1.6.

A major limitation of the DDD approach is that it is difficult to incorporate all the policy details. Because there were different roll outs of the Medicaid expansion, there are multiple post period delimiters and consequently multiple types of expansion states. Additionally, the relevant income thresholds differ for pre-ACA state plans and the Marketplace subsidies differ between expansion and non-expansion states.

1.5.2 Simulated Eligibility

Given the limitations with the DDD approach, I turn to a simulated eligibility approach to construct variables of interest similar to the ones in the DDD approach, but which allow for more precision and sources of variation. Based on Currie and Gruber (1996a, 1996b, 2001), this method starts with stratifying a national sample into demographic groups. Then the share of each group that would be eligible for the program is calculated based on income thresholds for each state and month. This measure of the share eligible can be thought of as reflecting the generosity of subsidized insurance programs in a state in a particular month. DeLeire et al. (2011) and Zavodny and Bitler (2010) used this method to estimate how the Medicaid eligibility expansions for pregnant women in the 1980s affected birth rates.

I adopt this approach, basing my demographic groups off the ones used in Currie and Gruber (2001) and subsequently used by DeLeire et al. (2011). I start with four race and ethnicity categories: non-Hispanic white, non-Hispanic black, non-Hispanic other, and Hispanic. In each race category, I divide women among 4 age groups: 15-18, 20-24, 25-34, and 35-45. Among non-teenagers, I classify women into 4 education groups: those with less than a high school diploma or GED, with a high school diploma/GED, with some college or an AA degree, and with a bachelor's degree or higher. For women with a high school degree or higher, I differentiate between those who are married or not. This leads to a total of 88 demographic bins, 22 for each race. A flowchart describing the construction of these demographic groups is in Figure A.2 of the appendix.

Using a national sample to calculate the share eligible avoids any variation coming from state specific demographics which may be correlated with the birth rate. For example, an eligibility measure based on a state's population would cause a poor state to have more people eligible as well as a higher birth rate. Using the national sample to impute eligibility makes the measure more reflective of the policy expansions alone. The issue of state variation is additionally controlled for by using state-by-group fixed effects.

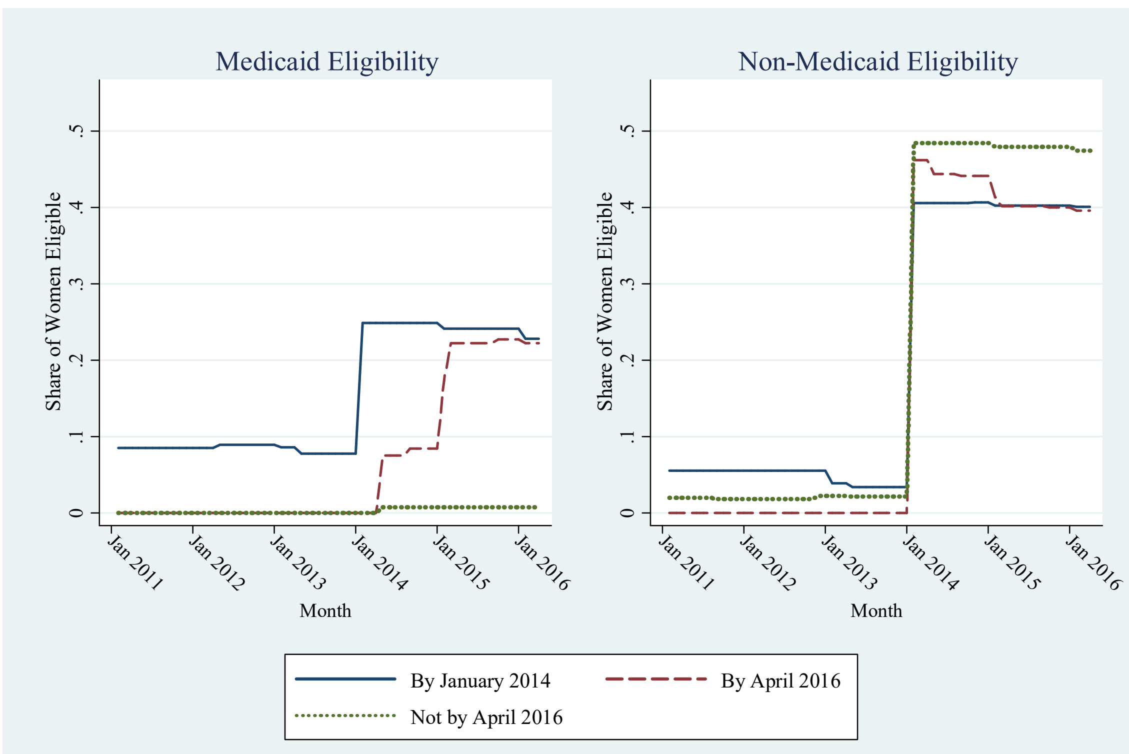
With this approach and the ACS data, I create one eligibility measure for Medicaid and another for NMSI plans. For example, childless adults in Nevada had no access to Medicaid or NMSI prior to the ACA, so both eligibility measures would be 0, because 0% of the national sample would be eligible for Medicaid and 0% were eligible for subsidized insurance in Nevada. When Nevada implemented the Medicaid expansion on January 1, 2014, adults at or below 138%FPL were eligible for Medicaid. The Medicaid eligibility variable for "Nevadan, white women, 20-24, with high school degrees, and married" is the share of white women, 20-24, with high school degrees, and married nationally who are at or below 138%FPL. Similarly, the NMSI eligibility measure for this group would be the share of white women, 20-24, with high school degrees, and married nationally who were between 139 and 400%FPL, since that is the income range where Nevada adults would be eligible for subsidies. Appendix A.1 provides more information on these measures.

For the simplest case of the Medicaid expansion, the simulated Medicaid eligibility measure and the triple interaction variable shown in equation (1) are essentially identical.[8] The benefits of the simulated eligibility measure arise when modeling the non-standard expansion cases. Given the generally congruous nature of these variables of interest, my preferred

[8] The only difference is that triple-difference variable uses the share below 138%FPL calculated for the demographic group in the state of the observation whereas the simulated eligibility measure uses the share below 138%FPL in that demographic group nationally.

specification is to replace the variables of interest, $PostXExpanderXBelow138_{gsy}$ and $PostX139to400_{gsy}$, from the DDD approaches with the simulated eligibility measures.

Figure 1.2 graphs changes in these eligibility measures by state expansion status. The share of women eligible for Medicaid is shown in the left panel. States that had waiver-based Medicaid plans or other childless adult plans were more likely to expand Medicaid by January 2014. For those expansion states, the share eligible increases to around 25% while it remains 0% for states which did not expand. The right panel shows the share eligible for NMSI. Marketplace subsidies apply nationally, so all states see an increase in eligibility in January 2014.



Source: ACS data

Figure 1.2 Medicaid and NMSI Eligibility Increases by State Expansion Date

The conceptual framework underlying this analysis is that increasing eligibility increases insurance enrollment, which may in turn affect the birth rate. Increases in insurance coverage could impact the birth rate by improving access to prescription-based contraception (less births) or increasing families' ability to afford children (more births). Either way, eligibility should not have an effect except through increased enrollment. Consequently, I start by estimating the effect of subsidized insurance eligibility on enrollment as given by equation (2).

$$insurance_{gsy} = \beta_0 + \beta_1 Medicaid_{gsy} + \beta_2 NonMedicaid_{gsy} + \boldsymbol{X}_{gsy}\beta_3 + \delta_{gs} + \delta_y + \varepsilon_{gsy} \quad (2)$$

The term $insurance_{gsy}$ is a variable measuring the share of women in group g in state s in year y who are enrolled in that type of insurance. I look at three insurance outcome variables: any insurance, Medicaid, and non-Medicaid insurance. The variables $Medicaid_{gsy}$ and $NonMedicaid_{gsy}$ are the simulated eligibility measures for the share of women in group g in state s in year y eligible for Medicaid or NMSI (meaning state subsidized childless adult plans or Marketplace plans). Because the data in the ACS is reported annually, I use the average eligibility over the course of the year. The eligibility measures are continuous variables from 0 to 1 so the coefficients β_1 and β_2 reflect the percentage point change in the share insured from a 100 percentage point increase in eligibility. Since the actual change in eligibility was closer to 9 percentage points for Medicaid and 40 percentage points for NMSI eligibility, I will rescale the coefficients to reflect a 10 percentage point change in eligibility when discussing the results. The variable $\boldsymbol{X}_{gsy}$ contains the controls from the DDD approach: $PostXExpander_{gsy}$, $PostXBelow138_{gsy}$, and UR_{sy}. As with the DDD approach, the model includes state-by-group fixed effects and year fixed effects, uses heteroscedasticity-robust standard errors, clusters at the state level, weights by population, and is ran separately by age group.

It is important to include both measures of eligibility in the same regression because they are correlated and failing to do so would result in omitted variable bias. The Marketplace expansion was rolled out nationally in 2014 which may suggest that adding the year dummy for 2014 would absorb the effect of the Marketplace and not require it to be modeled specifically; however, the size of the Marketplace expansion differs in states by whether they expanded Medicaid. In states which expanded Medicaid, the Marketplace subsidies exist for those 139-400%FPL whereas in non-expansion states subsidies are available for those 100-400%FPL. This regression shows the impact of eligibility for subsidized insurance, coming from changes in Medicaid (β_1) and non-Medicaid (β_2) eligibility, on insurance enrollment relative to not being eligible for subsidized insurance.

After verifying that eligibility increases insurance coverage, I test how eligibility for subsidized insurance affects the birth rate. I estimate the effect of eligibility on births using monthly data with equation (3):

$$\begin{aligned} \ln(birthrate)_{gsm} = \beta_0 + \ Medicaid_{gs(m-9)} + \beta_2 NonMedicaid_{gs(m-9)} \\ + \boldsymbol{X}_{gsy}\beta_3 + \delta_{gs} + \delta_m + \varepsilon_{gsm} \end{aligned} \tag{3}$$

Here, $ln(birthrate)_{gsm}$ measures the natural log of births per 1,000 women in group g in state s in month m. To handle observations where the birth rate is zero and have a balanced panel, I add 1/12 to the birth rate before taking the natural log as this is equivalent to adding 1 to the annual birth rates. Since groups that have zero births tend to be demographically small, they receive little weight in the analysis and results are insensitive to the amount added and whether the zero birth groups are included at all. The eligibility measures, $Medicaid_{gs(m-9)}$ and $NonMedicaid_{gs(m-9)}$, are now lagged by 9 months from the birth month to account for gestational age and thus reflect eligibility at the time of conception. I again include group-by-state fixed effects, but because the level of analysis is now monthly, I include month-by-year fixed effects. The controls $\boldsymbol{X}_{gsm}$ are monthly versions of the controls $\boldsymbol{X}_{gsy}$ in equation (2). Since I am using the natural log of the monthly birth rate, the coefficients of interest β_1 and β_2 approximate the percent change in the monthly birth rate from an increase in Medicaid and non-Medicaid eligibility respectively.

1.5.3 Two-Stage Least Squares

Understanding how eligibility affects insurance enrollment and the birth rate are both of interest in their own rights, but understanding the combined effect of how enrollment affects the birth rate is also important. This provides a useful alternative perspective to the eligibility effects because it accounts for the actual take-up of the policies. To do this, I use a two-stage least squares methodology where the eligibility measures serve as instruments for insurance enrollment. This estimates the effect of increases in insurance enrollment stemming policy changes which are arguably more exogenous than individuals' enrollment decisions. Simply using the shares of women enrolled may reflect the decisions of women which could be correlated with pregnancy decisions.

For the two-stage least squares methodology to be valid, the exclusion restriction that the birth rate is not effected by eligibility except through insurance enrollment must hold. It is important, therefore, to run the two-stage least squares regression with both types of insurance as endogenous variables that are being instrumented for by the two measures of eligibility. Suppose instead that the effect of Medicaid enrollment on the birth rate was estimated without controlling for non-Medicaid insurance enrollment. If Medicaid eligibility crowds out non-Medicaid coverage which affects the birth rate, then Medicaid eligibility impacts the birth rate through a channel other than Medicaid enrollment and the exclusion restriction would be violated. However, by including both types of insurance in the model, this violation of the exclusion restriction is avoided. I estimate the two-stage least squares equation with two endogenous variables with the following system of equations:

$$MedicaidEnrol_{gsy} = \beta_0 + \beta_1 Medicaid_{gsy} + \beta_2 NonMedicaid_{gsy} + \boldsymbol{X}_{gsy}\beta_3 + \delta_{gs} + \delta_y + \varepsilon_{gsy} \tag{4}$$

$$NonMedicaidEnrol_{gsy} = \beta_4 + \beta_5 Medicaid_{gsy} + \beta_6 NonMedicaid_{gsy} + \boldsymbol{X}_{gsy}\beta_3 + \delta_{gs} + \delta_y + \varepsilon_{gsy} \tag{5}$$

$$\ln(birthrate)_{gsy} = \gamma_1 Med\widehat{icaid}Enrol_{gsy} + \gamma_2 NonMed\widehat{icaid}Enrol_{gsy} + \boldsymbol{X}_{gsy}\gamma_3 + \delta_{gs} + \delta_y + \varepsilon_{gsy} \tag{6}$$

Equations (4) and (5) estimate the effect of the two eligibility measures on each type of insurance, similar to equation (2). The estimated shares of women enrolled from (4) and (5) are then included in equation (6). Medicaid and non-Medicaid enrollment together in account for all types of insurance so equation (6) measures the effect of insurance on the birth rate. The coefficient γ_1 is the effect of Medicaid enrollment, holding all other types of insurance constant, relative to no insurance. Similarly, the coefficient γ_2 is the effect of non-Medicaid subsidized enrollment, holding Medicaid constant, relative to no insurance. Since I only have annual insurance information, I now use annual birth rates instead of monthly to make the level of analysis consistent across the two stages.

1.5.4 Detailed Eligibility which includes pregnancy-conditional Medicaid

The methods thus far have focused on subsidized eligibility for non-pregnant women, but it is also important to look at how these policies interact with pregnancy-conditional Medicaid policies. All women eligible for the Medicaid expansion and some of the women eligible for Marketplace subsidies were eligible for pregnancy-conditional Medicaid before the Medicaid expansion. For these women the price of maternity services is held constant, whereas women eligible for Marketplace subsidies but above the pregnancy-conditional Medicaid threshold saw the cost of maternity services fall.

To isolate this group, I construct a more detailed set of policy groups to reflect the interactions between these policies. Since the pregnancy-conditional Medicaid income threshold is always the same or higher than the Medicaid expansion, I construct four groups of progressively higher income women: 1). Always Medicaid Eligible 2). Medicaid Eligible when Pregnant, Ineligible Otherwise 3). Medicaid Eligible when Pregnant, NMSI Eligible Otherwise 4). Always NMSI Eligible. The first two groups can be understood as subgroups of the Medicaid eligibility group from earlier while the second two are similar to subgroups of the non-Medicaid subsidized eligibility. I replace the two eligibility share variables in equation (2) with these four and estimate the effects on Medicaid enrollment, non-Medicaid enrollment, and the annual birth rate.

1.5.5 Validity Checks

Identifying assumptions in the DDD approach are that if the ACA expansion did not occur, trends in insurance and the birth rate, and differences in those trends between expansion and non-expansion states, would not be correlated with the share below 138%FPL, conditional on other controls. While it is not possible to prove directly that the identifying assumptions associated with my analysis hold, I present three tests which corroborate the validity of my analysis.

For the first test I use an event study which interacts the policy related variables in the DDD approach with each year of the sample using 2013 as a base year, following (Charles Courtemanche et al., 2017). I do this test at the annual level for the Medicaid insurance enrollment, the non-Medicaid insurance enrollment, and the birth rate. The idea is that these variables take on a significant effect when they become related to the eligibility policies, but should not have a significant effect during the pre-period. Specifically, I interact: the share below 138%FPL, expansion states interacted with the share below 138%FPL, and the share 139 to 400%FPL.

Next, I construct a placebo test where I lag my eligibility measures and then run them on the pre-period when they should not have an effect. For the insurance results, I lag by 1, 2, and 3 years. Since a 3 year lag makes the placebo enactment year 2011, I need to extend the pre-period backwards. In this test I use data from 2008 through 2013 to maintain the same analysis length as the main results of 6 years. For the birth rates outcomes, I use monthly data on conceptions from 2010 through 2013 and lag by 12, 18, and 24 months.

The third test uses teenage women who were mostly unaffected by the ACA expansions as a falsification group. Prior to the ACA, children up to age 18 belonged to a specific Medicaid eligibility category and many states had Children's Health Insurance Plan (CHIP) programs for children in families whose income exceeds Medicaid income thresholds. Following equations (2) and (3), I test whether the eligibility for older, but demographically similar, women affected the insurance enrollment of teens and their birth rate. I match demographic groups of women 15-18 to similar women 20-24 by their race and state and assign the eligibility variables of the older women to the teens. I do two versions of this test using different groups of women who may be similar to teens when they become 20: women 20-24 without a high school diploma/GED or unmarried women 20-24 with a high school diploma/GED. The birth rates for these groups are shown in Figure A.6 to demonstrate similarities in their trends prior to the ACA.

The Medicaid eligibility expansions for adults did not apply to teens, so teenage insurance and birth rates should not be correlated with Medicaid eligibility for young women. However, this approach is a less clean test for non-Medicaid subsidized eligibility. Teens who are above their state's Medicaid or CHIP cutoff may benefit from the Marketplace expansion. If the income distribution for teens and those 20-24 is similar, then the non-Medicaid subsidized eligibility measure for the older women may reflect the non-Medicaid eligibility for teens.

1.6 Results

In this section I start by reporting estimates of the effects of eligibility on enrollment and the birth rate respectively. Now I present the estimated effects of enrollment on the birth rate from the 2SLS approach. I then show the effect of more detailed eligibility categories on insurance take up and the birth rate. Moving into robustness checks, I present results from the DD and DDD approaches. Finally, I discuss the results from the event study, placebo test, and falsification test. All of these models are estimated separately by age group.

1.6.1 Insurance Enrollment Results

Table 1.2 displays the results from estimates of equation (2) describing how eligibility for Medicaid and non-Medicaid subsidized health insurance affect insurance enrollment. For each of the three insurance outcomes, I run the model without and then with the DDD controls, $PostXExpander_{gsy}$ and $PostXBelow138_{gsy}$, which gives model (1) and model (2) respectively. These controls are generally significant and attenuate the magnitudes of the simulated eligibility coefficients. Model (2) is the preferred specification which I will focus on while presenting the results, and Model (1) serves as a comparison. There are three panels of results for the three different age groups. The number of observations reflects the number of demographic groups by state by year minus the groups-by-states which did not appear in the ACS data.

The first two columns display the effect of subsidized insurance eligibilities on the share of women who have health insurance of any type. Across all age groups, both types of eligibility have positive and significant effects on the share of women insured. For women 20-24, a 10 percentage point increase in the share of women eligible for Medicaid would produce a 0.6

Table 1.2 Eligibility Effects on Enrollment

	Any Insurance Enrollment		Medicaid Enrollment		Non-Medicaid Enrollment	
	(1)	(2)	(1)	(2)	(1)	(2)
Females Aged 20-24						
Medicaid Eligibility	0.116***	0.0632**	0.142***	0.0938***	-0.0258	-0.0306
	(0.028)	(0.022)	(0.027)	(0.017)	(0.016)	(0.017)
NMSI Eligibility	0.0907*	0.118**	-0.0012	0.0263	0.0919**	0.0919***
	(0.039)	(0.037)	(0.027)	(0.026)	(0.027)	(0.025)
Unemployment	-0.00216	-0.000771	-0.00409	-0.00229	0.00193	0.00152
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Post X Expander State		0.0295***		0.0344***		-0.00482
		(0.007)		(0.006)		(0.006)
Post X Below 138%		0.195***		0.0555*		0.139***
		(0.025)		(0.025)		(0.021)
Group by State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
N=	6,230	6,230	6,230	6,230	6,230	6,230
Females Aged 25-34						
Medicaid Eligibility	0.212***	0.126***	0.213***	0.127***	-0.0015	-0.000993
	(0.033)	(0.028)	(0.033)	(0.020)	(0.022)	(0.021)
NMSI Eligibility	0.160***	0.103***	0.0484**	0.0465**	0.111***	0.0565**
	(0.027)	(0.023)	(0.016)	(0.016)	(0.020)	(0.020)
Unemployment	-0.000661	-0.00174	-0.000363	0.00104	-0.000299	-0.00278
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
Post X Expander State		0.0121		0.0374***		-0.0254***
		(0.006)		(0.004)		(0.005)
Post X Below 138%		0.152***		0.0717***		0.0802***
		(0.024)		(0.019)		(0.017)
Group by State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
N=	6,676	6,676	6,676	6,676	6,676	6,676
Females Aged 35-45						
Medicaid Eligibility	0.217***	0.162***	0.245***	0.168***	-0.0282	-0.00627
	(0.024)	(0.021)	(0.034)	(0.027)	(0.028)	(0.027)
NMSI Eligibility	0.137***	0.0956***	0.0292	0.0211	0.108***	0.0745***
	(0.022)	(0.023)	(0.019)	(0.022)	(0.020)	(0.017)
Unemployment	-0.00335	-0.00388	-0.00177	-0.000675	-0.00158	-0.00321
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Post X Expander State		0.00942		0.0336***		-0.0242**
		(0.007)		(0.006)		(0.007)
Post X Below 138%		0.0886***		0.0622**		0.0264
		(0.018)		(0.020)		(0.017)
Group by State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
N=	6,591	6,591	6,591	6,591	6,591	6,591

Notes: Data covers insurance outcomes from 2011-2016 for childless women 20-45. Regressions are run separately for each age group, so each panel represents the coefficients of interest from separate regressions. The regressions are weighed by state-by-group population and clustered by state. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

percentage point increase in the share insured, whereas a 10 percentage point increase in the share eligible for NMSI would produce a 1.2 percentage point increase in the share insured. These two coefficients do not significantly differ from each other (p=.183). For older women, the effects on the probability of any insurance are higher for NMSI eligibility, and the coefficients for the two eligibility types are significantly different for women 35-45 (p=.0367).

"Any insurance" is then partitioned into Medicaid and non-Medicaid insurance for the remaining two outcomes. As expected, Medicaid eligibility has a positive and significant effect on the share Medicaid enrolled whereas non-Medicaid subsidized eligibility has a positive and significant effect on non-Medicaid insurance enrollment. The effects of Medicaid eligibility on Medicaid enrollment increase as the age groups get older.[9] A 10 percentage point increase in Medicaid eligibility increases Medicaid enrollment by 0.9 percentage points for women 20-24, 1.3 percentage points for women 25-34, and 1.7 percentage points for women 35-45. In general, there is no clear effect of non-Medicaid eligibility on Medicaid enrollment, but non-Medicaid eligibility does have a positive and significant effect for women 25-34.

Increasing the share eligible for NMSI 10 percentage points increases the share of women with non-Medicaid insurance 0.9 percentage points for women 20-24, 0.6 percentage points for those 25-34, and 0.7 percentage points for those 35-45. There are several reasons the coefficients for NMSI eligibility on non-Medicaid Insurance are smaller than the coefficients for Medicaid eligibility on Medicaid enrollment. First, Medicaid is fully subsidized which makes it free to participate while non-Medicaid subsidized insurance is partially subsidized so enrollees do incur some costs which might dissuade enrolling. Additionally, to qualify for Marketplace subsidies people cannot have access to other affordable insurance such as ESI. The simulated NMSI

[9] The coefficients for Medicaid eligibility do not differ significantly between women 20-24 and women 25-34 (p=.132), but the coefficients between 20-24 year olds and 35-45 year olds do differ significantly (p=.021).

eligibility measure is based solely on income and thus overstates eligibility by not excluding people with ESI. While eligible women can enroll in Medicaid at any time, the Marketplace portion of NMSI eligibility has enrollment periods. Finally, higher income women are more likely to have ESI, which while included in the non-Medicaid insurance outcome, will not respond to NMSI eligibility. The results do not indicate that Medicaid significantly crowded out private insurance. The Medicaid eligibility coefficients in the non-Medicaid enrollment regressions are negative, but never significant. This evidence of limited crowd-out is consistent with other studies on the Medicaid expansion (Charles Courtemanche et al., 2017; Frean et al., 2017; Kaestner et al., 2017).

1.6.2 Birth Rate Results

Table 1.3 reports the estimates for the effects of eligibility on the natural log of the monthly birth rate from separate regressions for each age group. Again the DDD controls attenuate the results, but the signs of coefficients remain the unchanged. In the preferred specification (column 2), Medicaid eligibility has no significant effect on the monthly birth rate for any of the age groups; however, NMSI eligibility has a significant positive effect on the monthly birth rate.

The lack of significant changes in the birth rate from increased Medicaid eligibility is unexpected given the success of family planning waivers have had in reducing the birth rate by providing free contraception to low-income adults. The cost of maternity services is held constant for this group since they were eligible for pregnancy-conditional Medicaid before and after the Medicaid expansion, so Medicaid eligibility largely isolates the contraception effects of subsidized insurance. However, these effects are not very precisely estimated since a 10 percentage point increase in Medicaid eligibility produces between a -0.4% and 1.4% change in the birth rate for women 20-25 and between a -1.2% and 1.1% change for women 35-45.

Table 1.3 Eligibility Effects on Monthly Birth Rate

	ln(monthly birthrate +1/12)	
	(1)	(2)
Females Aged 20-24		
Medicaid Eligibility	0.0174	0.05
	(0.036)	(0.047)
NMSI Eligibility	0.0740*	0.0552*
	(0.034)	(0.025)
Unemployment	-0.00742	-0.00875
	(0.006)	(0.006)
Post X Expander State		-0.03
		(0.019)
Post X Below 138%		0.0958**
		(0.033)
Group by State FE	Y	Y
Year FE	Y	Y
N=	76,216	73,138
Females Aged 25-34		
Medicaid Eligibility	0.0620*	0.00332
	(0.027)	(0.039)
NMSI Eligibility	0.130***	0.0742*
	(0.026)	(0.028)
Unemployment	-0.00365	-0.00527
	(0.003)	(0.003)
Post X Expander State		0.000414
		(0.007)
Post X Below 138%		0.129*
		(0.057)
Group by State FE	Y	Y
Year FE	Y	Y
N=	76,216	74,973
Females Aged 35-45		
Medicaid Eligibility	0.0366	-0.00384
	(0.041)	(0.059)
NMSI Eligibility	0.201***	0.146**
	(0.057)	(0.042)
Unemployment	-0.00011	-0.00141
	(0.006)	(0.005)
Post X Expander State		-0.00289
		(0.014)
Post X Below 138%		0.0948
		(0.073)
Group by State FE	Y	Y
Year FE	Y	Y
N=	76,216	74,034

Notes: Data covers births conceived from 2011-2016 for childless women 20-45. Regressions are run separately for each age group, so each panel represents the coefficients of interest from separate regressions. The regressions are weighed by state-by-group population and clustered by state. *$p < .05$, ** $p < .01$, *** $p < .001$

For women 20-24, a 10 percentage point increase in NMSI eligibility would raise the monthly birth rate by a marginally significant 0.56%. Scaling this by the actual 43.8 percentage point change in non-Medicaid eligibility for this group suggests that the increase in this type of eligibility increased the monthly birth rate by 2.45% for young women. The size of these effects increase with age, though they are not significantly different (comparing the coefficients with the 20-24 group, p=.658 for 25-34 and p=.085 for 35-45). For women 35-45, a 10 percentage point increase in eligibility would lead to a 1.46% increase in monthly births. Scaled by the 38.7 percentage point increase in eligibility for this group, this represents a 5.65% increase in the monthly birth rate for older women.

The magnitude of these increases in the birth rate is fairly substantial, but not unreasonable. The pregnancy-conditional Medicaid expansion during the 1980s which reduced maternity costs did not produce robust increases across births, but the estimates varied widely. The baseline results in (DeLeire et al., 2011) found a 10 percentage point increase in the share eligible led to a 12% increase in the birth count for whites and a 24% increase for African-Americans, though the magnitude and significance of those effects went away after adding cell-by-state fixed effects, which I include. The results in Zavodny and Bitler (2010) are similar in magnitude to mine. They find a 10 percentage point increase in the share eligible led to a 0.7% increase in the birth rate among whites and a 0.1% reduction among blacks, though these results were insignificant. Studies have found even larger increases in births following gains in private insurance, though the situations are less comparable. The RAND study found women with free maternity services had 29% higher births than those with a copay and an HMO in California had twice as many births among new enrollees than ones who had been enrolled more than a year.

Given the difficulty finding coverage for maternity services on the non-group market prior to the Marketplace expansion, there was likely some pent-up demand for children. While not significantly different than younger women, the larger effects of NMSI eligibility for older women may reflect that women nearing the end of their childbearing years are more responsive to the subsidized insurance changes.

1.6.3 Enrollment on the Birth Rate

This section presents the 2SLS estimates of how enrollment into Medicaid and non-Medicaid insurance affects the birth rate. Both types of insurance are included in the regressions as endogenous variables which are instrumented for using the two constructed measures of eligibility. The uppers section of each age group panel provides the Sanderson-Windmeijer multivariate F-statistics. The instruments perform well as indicated by SW F-statistics exceeding 10 for all insurance enrollments and age groups with the exception of non-Medicaid insurance for women 25-34 where it is 6.84. For this exception, the SW F-statistic is 30.64 without the DDD controls which provides some reassurance that the instruments in general do explain non-Medicaid insurance enrollment, but that the additional controls absorb some explanatory power.

The outcome variable in the regression is the natural log of the annual birth rate so the coefficients can be interpreted loosely as percent changes. I add 1 to the birth rate before taking the logarithm in order to have a balanced panel, though this makes little difference to results. Table 1.4 shows that across all age groups, Medicaid enrollment has no statistically significant effect on the annual birth rate while non-Medicaid enrollment has a significant and positive effect. For women 20-24, a 10 percentage point increase in non-Medicaid enrollment stemming from increased eligibility for subsidized insurance leads to a 7.8% increase in the annual birth rate. The

Table 1.4 Enrollment Effects on the Annual Birth Rate

	ln(birthrate +1)	
	(1)	(2)
Females Aged 20-24		
Medicaid Enrollment SW F-test	19.73	10.53
Non-Medicaid Enrollment SW F-test	30.580	15.670
Medicaid Enrollment	0.349	0.836
	(0.373)	(0.492)
Non-Medicaid Enrollment	1.298***	0.780**
	(0.347)	(0.280)
Unemployment	-0.00825	-0.00858
		(0.00759)
Post X Expander State		-0.0622
		(0.033)
Post X Below 138%		-0.0414
		(0.058)
Group by State FE	Y	Y
Year FE	Y	Y
N=	6,164	6,164
Females Aged 25-34		
Medicaid Enrollment SW F-test	40.46	28.53
Non-Medicaid Enrollment SW F-test	30.640	6.840
Medicaid Enrollment	0.325	0.123
	(0.167)	(0.429)
Non-Medicaid Enrollment	1.267***	1.612**
	(0.209)	(0.625)
Unemployment	-0.00463	-0.00255
	(0.004)	(0.005)
Post X Expander State		0.0325
		(0.028)
Post X Below 138%		-0.00732
		(0.119)
Group by State FE	Y	Y
Year FE	Y	Y
N=	6,637	6,637
Females Aged 35-45		
Medicaid Enrollment SW F-test	107.17	33.3
Non-Medicaid Enrollment SW F-test	64.060	20.840
Medicaid Enrollment	0.306	0.129
	(0.208)	(0.442)
Non-Medicaid Enrollment	1.667***	1.941***
	(0.329)	(0.488)
Unemployment	0.001	0.002
	(0.007)	(0.007)
Post X Expander State		0.0343
		(0.032)
Post X Below 138%		0.00443
		(0.086)
Group by State FE	Y	Y
Year FE	Y	Y
N=	6,554	6,554

Notes: Data covers insurance outcomes and births conceived from 2011-2016 for childless women 20-45. Regressions are run separately for each age group, so each panel represents the coefficients of interest from separate regressions. The regressions are weighed by state-by-group population and clustered by state. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

effect of non-Medicaid enrollment increases with age. For women 35-45, the same increase in non-Medicaid enrollment would lead to a 19.4% increase in the annual birth rate.

Another way to understand these results is to scale them by the percentage point change in enrollment that occurred around the ACA insurance expansions. Before and after 2014 there was a 5.2 percentage point increase in the share of 20-24 women enrolled in non-Medicaid insurance. Scaling the two-stage least squares estimates by this number suggests that the ACA expansion would have increased annual births by 4.1% for those 20-24. Non-Medicaid insurance among women 35-45 increased by 3.4 percentage points, which would suggest a 6.6% increase in annual births.

1.6.4 Detailed Eligibility Measures:

Going back to the effects of eligibility, Table 1.5 presents the results for more detailed eligibility groups which take into consideration both the subsidized insurance eligibility opportunities for non-pregnant women *and* the pregnancy-conditional Medicaid eligibility for pregnant women. The results for Medicaid enrollment, in the first two columns, indicate that the positive enrollment effect of Medicaid eligibility is concentrated among those who are always Medicaid eligible, regardless of if they are pregnant (Group 1). As in Table 1.2, the size of the effect increases with age group, and again these coefficients only differ significant between 20-24 year olds and 35-45 year olds (p=.008).

For non-Medicaid insurance enrollment, the positive enrollment effects are concentrated among the women Always NMSI Eligible (Group 4). The coefficients for the share in Group 4 are positive across all age groups and significant for the oldest two. The fact that the Medicaid eligibility effect is concentrated among the lowest income group and the Marketplace eligibility effect is concentrated among the highest income group is what was expected, so these results corroborate that eligibility is working through the indented insurance channels in the model. Note that less eligibility coefficients are significant here than in Table 1.2 which used the two eligibility measures. This suggests that detecting results is harder with these noisy and correlated eligibility measures.

Table 1.5 Detailed Eligibility Results

	Medicaid Enrollment		Non-Medicaid Enrollment		ln(birthrate +1)	
Aged 20-24	(1)	(2)	(1)	(2)	(1)	(2)
Always Medicaid Eligible	0.0857 (0.047)	0.0868* (0.0359)	-0.130* (0.063)	-0.103 (0.0539)	-0.0497 (0.125)	-0.0116 (0.1260)
Medicaid when Pregnant, Ineligible Otherwise	-0.0545 (0.041)	-0.00279 (0.0384)	-0.112 (0.071)	-0.0807 (0.0620)	-0.0501 (0.118)	-0.0485 (0.1160)
Medicaid when Pregnant, NMSI Eligible Otherwise	-0.0692 (0.040)	0.000554 (0.0351)	-0.00469 (0.040)	0.0108 (0.0384)	-0.0121 (0.126)	-0.0474 (0.1290)
Always NMSI Eligible	-0.0106 (0.035)	0.0417 (0.0315)	0.00874 (0.037)	0.0346 (0.0359)	0.189** (0.058)	0.183** (0.0593)
Unemployment	-0.00483 (0.003)	-0.0031 (0.0031)	0.00169 (0.003)	0.000904 (0.0026)	-0.0102 (0.009)	-0.0126 (0.0085)
Post X Expander State		0.0338*** (0.0059)		-0.00743 (0.0063)		-0.0386* (0.0177)
Post X Below 138%		0.0590* (0.0279)		0.127*** (0.0213)		0.125** (0.0452)
N=	6,230	6,230	6,230	6,230	7,448	7,147
Aged 25-34						
Always Medicaid Eligible	0.167* (0.081)	0.147* (0.0572)	-0.137* (0.052)	-0.105* (0.0439)	0.0507 (0.114)	0.0681 (0.0842)
Medicaid when Pregnant, Ineligible Otherwise	-0.0495 (0.072)	0.0257 (0.0615)	-0.147** (0.051)	-0.112* (0.0466)	-0.0266 (0.116)	0.0527 (0.0762)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.00347 (0.049)	0.0522 (0.0398)	-0.0313 (0.052)	-0.0701 (0.0500)	0.173** (0.064)	0.156* (0.0590)
Always NMSI Eligible	0.0172 (0.029)	0.0466 (0.0240)	0.105*** (0.028)	0.0850** (0.0275)	0.0774 (0.054)	0.073 (0.0509)
Unemployment	-0.000536 (0.003)	0.000745 (0.0022)	-0.000795 (0.003)	-0.00339 (0.0021)	-0.00514 (0.005)	-0.00714 (0.0044)
Post X Expander State		0.0373*** (0.0043)		-0.0255*** (0.0053)		-0.0042 (0.0072)
Post X Below 138%		0.0804** (0.0229)		0.0766*** (0.0169)		0.130** (0.0460)
N=	6,676	6,676	6,676	6,676	7,448	7,325

Table 1.5 Detailed Eligibility Results Continued

	Medicaid Enrollment		Non-Medicaid Enrollment		ln(birthrate +1)	
Aged 35-45						
Always Medicaid Eligible	0.252*** (0.065)	0.231*** (0.0521)	-0.054 (0.068)	-0.0346 (0.0592)	0.146 (0.137)	0.155 (0.1300)
Medicaid when Pregnant, Ineligible Otherwise	0.00791 (0.054)	0.0775 (0.0509)	-0.0267 (0.065)	-0.0249 (0.0644)	0.117 (0.144)	0.159 (0.1280)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.026 (0.038)	0.064 (0.0450)	0.0629 (0.050)	0.0215 (0.0512)	0.306* (0.124)	0.277* (0.1200)
Always NMSI Eligible	0.0256 (0.035)	0.0434 (0.0281)	0.122** (0.035)	0.0983* (0.0368)	0.160* (0.072)	0.141 (0.0777)
Unemployment	-0.00195 (0.003)	-0.000949 (0.0026)	-0.00202 (0.004)	-0.0038 (0.0031)	-0.00234 (0.007)	-0.00407 (0.0068)
Post X Expander State		0.0338*** (0.0059)		-0.0243** (0.0071)		-0.00895 (0.0143)
Post X Below 138%		0.0769*** (0.0213)		0.0317 (0.0170)		0.085 (0.0604)
N=	6,591	6,591	6,591	6,591	7,448	7,237

Notes: Data covers insurance outcomes and births conceived from 2011-2016 for childless women 20-45. Regressions are run separately for each age group, so each panel represents the coefficients of interest from separate regressions. All regressions contain the state-by-group fixed effects and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

Using the detailed eligibility measures for birth rates allows the analysis to hone in on the share Always NMSI Eligible (Group 4). Women in this group are ineligible for pregnancy-conditional Medicaid unlike the other three groups, and thus experienced the biggest reduction in costs of maternity services. The expectation is that the increase in the birth rate should be driven by a higher share of women in Group 4. That holds for women 20-24. However, for the two groups of older women the increase in births stems from the share Medicaid Eligible when Pregnant, Non-Medicaid Eligible Otherwise (Group 3) instead While these results do not exactly meet expectations, it is encouraging that the coefficients for Group 4 are positive across all age groups.

1.6.5 Alternative Specification: DD and DDD

Table 1.6 presents results from the DD and DDD approaches for Medicaid enrollment, non-Medicaid enrollment, and the annual birth rate. For each of these outcomes I present a DD model which just focuses on the Medicaid expansion, then a DD approach looking at the Medicaid expansion from equation (2), and then a DDD approach which also incorporates the share 139 to 400%FPL starting in 2014 to capture the Marketplace expansion.

The first column indicates that Medicaid enrollment increased between 4.6 and 5.2 percentage points in expansion states relative to non-expansion states following January 2014. As discussed earlier, the triple interaction between expansion states, the post period, and the share below 138%FPL is roughly congruous to the simulated Medicaid eligibility variable which means the coefficients should be comparable, and they are. The coefficient of the triple interaction term is identical for women 20-24 (.142) and very similar for older women (triple interaction coefficients are .149 and .170 for women 25-34 and 35-45 respectively compared to the Medicaid eligibility coefficients of .127 and .168 for these age groups). Adding in the interaction between the post period and the share between 139 and 400%FPL (column 3) does not significantly attenuate the Medicaid triple interaction term, though it loses some significance among the youngest age group.

For the non-Medicaid insurance enrollment results, the interaction between post and the share between 139 and 400%FPL differs more from the NMSI eligibility variable than the triple interaction term did with the Medicaid eligibility variable. The interaction term between post and the share between 139 and 400%FPL are not nearly identical with the NMSI eligibility, but they are similar. Both are positive and significant and show a larger effect for women 20-24 compared to the older women. Again there is slight evidence that Medicaid, as indicated by the triple interaction term, crowds out non-Medicaid insurance, but the effects are only significant among the youngest group.

Table 1.6 DD and DDD Results

	Any Insurance Enrollment			Medicaid Enrollment			Non-Medicaid Enrollment			ln(birthrate +1)		
	DD	DDD	DDD with Marketplace Controls	DD	DDD	DDD with Marketplace Controls	DD	DDD	DDD with Marketplace Controls	DD	DDD	DDD with Marketplace Controls
Aged 20-24												
Post X Expander	0.0235**	0.0115	0.0304*	0.0469***	-0.00023	0.00641	-0.0235***	0.0117	0.024	-0.0411*	-0.0353	-0.0261
	(0.008)	(0.015)	(0.014)	(0.008)	(0.015)	(0.016)	(0.007)	(0.015)	(0.014)	(0.017)	(0.036)	(0.034)
Post X Below 138%		0.171***	0.308***		0.00918	0.0571*		0.162***	0.251***		0.114*	0.184***
		(0.030)	(0.033)		(0.020)	(0.025)		(0.028)	(0.027)		(0.046)	(0.050)
Post X Expander X Below 138%		0.044	0.0146		0.142**	0.132*		-0.0979*	-0.117**		-0.0107	-0.0247
		(0.050)	(0.047)		(0.052)	(0.052)		(0.040)	(0.038)		(0.088)	(0.082)
Post X 139 to 400%			0.374***			0.131***			0.243***			0.187**
			(0.039)			(0.035)			(0.047)			(0.054)
N=	6,230	6,230	6,230	6,230	6,230	6,230	6,230	6,230	6,230	7,448	7,147	7,147
Females Aged 25-34												
Post X Expander	0.0142*	(0.009)	(0.001)	0.0457***	0.0174***	0.0203***	-0.0315***	-0.0263***	-0.0214***	(0.013)	0.016	0.0210*
	(0.006)	(0.006)	(0.007)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)	(0.009)	(0.010)
Post X Below 138%		0.176***	0.152***		0.0679***	0.0590**		0.108***	0.0934***		0.223***	0.209***
		(0.024)	(0.027)		(0.019)	(0.021)		(0.016)	(0.018)		(0.047)	(0.051)
Post X Expander X Below 138%		0.131***	0.114**		0.149***	0.142***		-0.0181	-0.0287		-0.133*	-0.143*
		(0.036)	(0.039)		(0.030)	(0.031)		(0.025)	(0.025)		(0.063)	(0.061)
Post X 139 to 400%			0.127***			0.0469**			0.0797***			0.0743
			(0.015)			(0.015)			(0.019)			(0.037)
N=	6,676	6,676	6,676	6,676	6,676	6,676	6,676	6,676	6,676	7,448	7,325	7,325
Females Aged 35-45												
Post X Expander	0.0196*	-0.00737	-0.00147	0.0517***	0.0159***	0.0188***	-0.0322***	-0.0233**	-0.0202**	-0.0225	0.0185	0.0232
	(0.008)	(0.006)	(0.005)	(0.008)	(0.004)	(0.005)	(0.008)	(0.007)	(0.007)	(0.016)	(0.019)	(0.019)
Post X Below 138%		0.130***	0.0954***		0.0555***	0.0387*		0.0743***	0.0567**		0.209**	0.181*
		(0.015)	(0.016)		(0.014)	(0.016)		(0.019)	(0.019)		(0.078)	(0.081)
Post X Expander X Below 138%		0.132***	0.124***		0.170***	0.166***		-0.0374	-0.0417		-0.180*	-0.186*
		(0.031)	(0.031)		(0.040)	(0.041)		(0.034)	(0.034)		(0.087)	(0.086)
Post X 139 to 400%			0.107***			0.0522**			0.0550**			0.0880*
			(0.018)			(0.016)			(0.020)			(0.043)
N=	6,591	6,591	6,591	6,591	6,591	6,591	6,591	6,591	6,591	7,448	7,237	7,237

Notes: Data covers insurance outcomes and births conceived from 2011-2016 for childless women 20-45. Regressions are run separately for each age group, so each panel represents the coefficients of interest from separate regressions. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. $^{*}p < .05$, $^{**} p < .01$, $^{***} p < .001$

The DDD approach again tells a similar story as the simulated eligibility results for birth rate outcomes. The interaction between the post period and the share between 139 and 400%FPL is positive which is consistent with the positive effect of non-Medicaid subsidized eligibility, though this coefficient is now insignificant for women 25-34. The triple interaction term is negative across all ages in both of the DDD approaches, and these coefficients are marginally significant for the older two groups. This contrasts with the positive and insignificant results found for Medicaid eligibility in Table 1.3. These negative coefficients on the triple interaction term are more indicative of Medicaid eligibility causing reductions in the birth rate, as would be expected from a contraception effect.

1.6.6 Validity Checks

The first validity test that I provide is an event study where the relevant policy variables are interacted with each year, using 2013 as a base, to see if they were significant before the expansion. The results from this specification can be found in Table A.4. There are 3 relevant variables lagged for 2011 and 2012 for three outcomes and three age groups, producing 54 variables of which should ideally be insignificant. Only 4 of these variables are significant, which is 7.4% and only slightly higher than the 5% expected by chance.

The next test is a placebo test to determine whether lagged eligibility variables are significant during the pre-2014 period. I run separate regressions for each of the three age lags for the two eligibility measure on three outcome variables for each of the three age groups for a total of 54 regressions in each table. The coefficients of interested are in Table A.5 and reveal 3 coefficients which are significant, which is 5.5% and therefore reassuring.

The results for the falsification test of how eligibility for women 20-24 affected teens 15-18 are presented in Table 1.7. The idea is that eligibility for slightly older, adult women should

Table 1.7 Teens as a Falsification Test

	Medicaid Enrollment	Non-Medicaid Enrollment	ln(monthly birth rate +1/12)
Eligibility for women 20-24 without high school diplomas/GEDs			
Medicaid Eligibility	-0.00121	-0.000866	0.00456
	(0.011)	(0.018)	(0.035)
NMSI Eligibility	-0.0218	0.0424*	0.0364
	(0.015)	(0.017)	(0.022)
Unemployment Rate	-0.000854	-0.00385	0.00152
	(0.004)	(0.003)	(0.005)
Expander State X Post	0.00884	-0.0102	0.00352
	(0.005)	(0.009)	(0.015)
Post X Below 138%	0.0301	0.000626	-0.0637
	(0.026)	(0.029)	(0.045)
N=	1,015	1,015	10,426
Eligibility for women 20-24 with high school diplomas/GEDs and not married			
Medicaid Eligibility	0.00554	-0.012	0.0145
	(0.014)	(0.019)	(0.050)
NMSI Eligibility	-0.0195	0.0624**	0.0552*
	(0.019)	(0.020)	(0.024)
Unemployment Rate	-0.000797	-0.00308	0.00234
	(0.004)	(0.003)	(0.005)
Expander State X Post	0.00767	-0.00461	0.00339
	(0.005)	(0.008)	(0.017)
Post X Below 138%	-0.0187	0.100*	-0.0598
	(0.033)	(0.042)	(0.076)
N=	1,060	1,060	10,888

Notes: Data covers insurance outcomes and births conceived from 2011-2016 for childless women 15-18. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

not affect the eligibility of the teen group, which has been eligible for Medicaid and CHIP to levels above the Medicaid expansion prior to the ACA. I "borrow" eligibility from the demographic groups "Women 20-24 Without High School Diplomas/GEDs" and "Women 20-24 with High School Diplomas/GEDs and Unmarried" which I match to the teens by race, state and time period.

The results using the eligibility from the first demographic group are in panel one and results using the eligibility of the second demographic group are in the panel below. Encouragingly, Medicaid eligibility supplied from either demographic group has no significant impact on any of the outcomes and passes the falsification test. The test of non-Medicaid eligibility is less clean. It appears the non-Medicaid eligibility from the women 20-24 increases enrollment in non-Medicaid insurance and the birth rate for teens. If non-Medicaid eligibility for subsidized insurance for adults causes the parents of teens to enroll in insurance, the enrollment for teens could legitimately improve, especially for teens between their state's Medicaid/CHIP threshold and 400%. The positive, significant effect of non-Medicaid subsidized eligibility for the teen birth rate is less reasonable. Given that 80% of teen pregnancies are unintended (L. B. Finer & Zolna, 2011), it is unlikely that cheaper childbearing encouraged teens to conceive, but it is possible that it led to more unintended pregnancies resulting in birth rather than abortion. Fortunately it is only significant for one of the donor demographic groups, and even then only marginally so. Overall, this falsification supports that there were not widespread changes in the outcomes of interest that were correlated with these eligibility measures when the eligibility policies did not apply.

1.7 Discussion and Policy Implications

The rate of uninsurance has been highest among women during their childbearing years. While Medicaid has existed for low-income pregnant women since the 1980s, childless adults have historically been ineligible for Medicaid. Additionally, for women without access to ESI, the non-group health insurance market was expensive, gender-rated, and rarely covered maternity services. The ACA greatly expanded subsidized health insurance opportunities for low-income childless adults by allowing states to expand Medicaid to childless adults and providing subsidies for non-group plans purchased through the Marketplace. I find that eligibility for Medicaid and NMSI both increased insurance among childbearing aged women, and that eligibility for NMSI increased the birth rate.

I model the impacts of increased eligibility for Medicaid and NMSI simultaneously using simulated eligibility measures. Given the similarities between the simulated Medicaid eligibility and a DDD between expansion states, the post period, and the share below 138%FPL, my preferred specification incorporates the two-way interaction control variables from the DDD framework. I demonstrate that not including these controls in the simulated eligibility model overstates the effects. While the results are comparable to those from a DDD approach, the simulated eligibility measures allow for modeling complex sources of variation, which are particularly important for the NMSI policies.

I find that the Medicaid and Marketplace eligibility expansions significantly increased insurance coverage, consistent with the ACA literature (Charles Courtemanche et al., 2017; C. Courtemanche et al., 2016; Frean et al., 2017; Kaestner et al., 2017). Increases in both Medicaid and NMSI eligibility each led to a significant increase in the share of women with any insurance. Specifically, a 10 percentage point increase in the share of women eligible for Medicaid increased the share enrolled in Medicaid between 0.9 and 1.6 percentage points across age groups, with higher take-up among older women. Medicaid eligibility has negative but insignificant effects on non-Medicaid insurance enrollment, suggesting Medicaid did not substantially crowd out private insurance. Take up of non-Medicaid insurance eligibility was smaller than for Medicaid eligibility, with a 10 percentage point increase in NMSI eligibility causing between a 0.6 and a 0.9 percentage point increase in non-Medicaid insurance enrollment.

Given previous studies' findings of reductions in birth rates following the implementation of family planning waivers, it seemed likely that eligibility for Medicaid, and thus contraception, would reduce the birth rate. However, Medicaid eligibility had no significant effect on the birth rate. Had it reduced the birth rate, states would have regained some cost savings on Medicaid-paid births by expanding Medicaid.

The primary finding of this chapter is that non-Medicaid subsidized eligibility significantly increases the birth rate. The effects are surprisingly large with a 10 percentage point increase in non-Medicaid eligibility producing between a 0.6% and 1.4% increase in the birth rate depending on the age group, with larger effects for older women. This magnitude is likely explained in part by a pent-up demand where women who had put off childbirth due to expenses are ready to take advantage of cheaper maternity services. The RAND study similarly found surprisingly large increases in births as a result of a temporary reduction in the price of medical care (Leibowitz, 1990). However, the introduction of pregnancy-conditional Medicaid in the 1980s, which also reduced maternity costs for eligible women, did not significantly increase the birth rate (DeLeire et al., 2011; Joyce et al., 1998; Zavodny & Bitler, 2010). The Marketplace expansion may be more comparable to the RAND study than the pregnancy-conditional Medicaid expansion given that Marketplace subsidies apply to pre-pregnancy, private insurance and women with slightly higher incomes.

The finding that NMSI increases the birth rate has several policy implications. While the U.S. is not facing fertility issues like Japan and parts of Europe, the U.S. birth rate is at a record low (Martin et al., 2018). These results suggest that subsidizing childbearing costs may slightly increase births among middle-income, childless women, at least in the short run. Additionally, this information is important for insurers offering Marketplace plans. From an insurer's perspective, the increase in births can be viewed as a moral hazard situation where women give birth when they have insurance who would not have given birth otherwise. The prenatal care and delivery expenses involved in a birth are expensive. If insurers underestimate the extent to which gaining subsidized, non-group insurance induces births, they will underestimate costs and not set premiums high enough.

This chapter has several limitations. First, I do not take into consideration changes in pregnancy outcomes other than births. While my preferred specification finds no indication that Medicaid eligibility reduced the birth rate, it is possible that improved contraception reduced abortions. Additionally, I focus on childless women who had fewer subsidized insurance options than parents did prior to the ACA. It is not clear how these results would extrapolate to the broader population of women. Finally, given the variety of ACA reforms, many of which applied to the non-group market and were contemporaneous, this analysis has focused on subsidized insurance as a package policy rather than isolating the different components. From a policy perspective, it would be more useful to know the separate impacts of prohibiting differential treatment of pre-existing conditions, providing contraception without cost sharing, mandatory coverage of maternity services, and prohibiting gender-rating.

CHAPTER 2. SUBSIDIZED INSURANCE AND PRENATAL CARE, MATERNAL HEALTH AND BIRTH OUTCOMES

2.1 Introduction

In an influential 1985 report, the Institute of Medicine (IOM) indicated racial and socio-demographic disparities in birth outcomes were not only substantial, they appeared to be growing. The report also raised concerns about disparities in the use of prenatal care which was primarily caused by financial constraints and inadequate insurance (Institute of Medicine, 1985). Against the backdrop of the largest share of uncompensated care at hospitals being birth related (Frank et al., 1990), this report helped influence the policy decision to expand Medicaid to broader groups of pregnant women in the late 1980s, with the expectation that coverage would increase the timeliness and use of prenatal care and subsequently improve birth outcomes (Alexander & Kotelchuck, 2001; Dave et al.; Institute of Medicine, 1985; Lu & Halfon, 2003). Yet, despite progress toward the goals of the Medicaid expansion for pregnant women, stark disparities remain in the share of preterm births and low birthweight (LBW) infants.

Medicaid is an important source of insurance for low-income pregnant women. Pregnant women below certain income thresholds receive fully subsidized care for health expenses related to their pregnancies, deliveries, and 60 days postpartum care. Prior to the ACA, in 2010, 43.2% of births to U.S women of childbearing age (15-44 years old) in the United States were covered by Medicaid (Vital Statistics (VS) data). Even though Medicaid provides free prenatal care for enrolled women, those women with Medicaid-paid births were twice as likely to have no prenatal care visits in the first trimester of pregnancy compared to those with births paid by using another type of insurance (36.7% compared to 15.2%, VS data). One reason for delays in accessing prenatal care may be that Medicaid coverage has been conditional on pregnancy. When women are ineligible prior to pregnancy, they may not be aware that becoming pregnant makes them eligible. Those who are aware may delay enrolling due to administrative barriers or unfamiliarity with welfare programs and health institutions.

As originally passed, the Affordable Care Act (ACA) was to expand Medicaid to all adults with incomes at 138% of the federal poverty line (FPL) and below, irrespective of pregnancy status. Furthermore, the ACA reformed the non-group insurance market by creating the Marketplace to facilitate the purchase of non-group insurance plans and provided subsidies to assist low-income individuals in affording such plans. Included in the non-group insurance reforms was the requirement that plans cover maternity services, which had previously been rare and expensive in non-group plans. While a Supreme Court ruling struck down the Medicaid expansion portion of the ACA and left the decision to expand Medicaid in the hands of the states, the Marketplace portion was left untouched.

The ACA's expansion of subsidized health insurance could impact medical care utilization, maternal health behaviors, and birth outcomes in several ways which differ slightly for Medicaid and Marketplace expansions within the ACA. First, women in the newly eligible adult Medicaid population would have been eligible for pregnancy-conditional Medicaid prior to the ACA, meaning their insurance eligibility during pregnancy has not changed. These women may still benefit from having newly subsidized insurance *prior* to conception because of greater awareness of their insurance eligibility, greater familiarity with medical institutions, and better pre-conception health. Second, women who are eligible for subsidized insurance through the Marketplace, but have incomes above the threshold for pregnancy-conditional Medicaid, also saw reductions in expenses related to maternity services. These reductions may also improve timeliness and adequacy of care. A third consideration is the potential for health insurance to create *ex-ante* moral hazard. If women in the newly eligible Medicaid adult population know they will have insurance beyond the 60 days postpartum period when pregnancy-conditional Medicaid ends, they may be less concerned with poor birth outcomes and engage in riskier health behaviors. This *ex-ante* moral hazard may exist to an even greater extent for women benefiting from Marketplace subsidies to pay for their pregnancies since they experienced larger increases in cost subsidies than women in the newly eligible Medicaid population.

This chapter examines how eligibility for Medicaid and non-Medicaid subsidized insurance (NMSI) affects prenatal care use, maternal behaviors during pregnancy, and birth outcomes. Medicaid is a fully subsidized form of insurance and NMSI is partially subsidized which includes state-plans prior to the ACA and Marketplace plans. The first chapter of this dissertation looked at how these forms of eligibility could affect the birth rate, and found non-Medicaid subsidized insurance eligibility led to significant increases in the birth rate. This chapter delves further into pregnancy related outcomes. This is the first paper to look at how the subsidized insurance portions of the ACA affected prenatal care, maternal health behaviors, delivery procedures, and birth outcomes.

Using Vital Statistics (VS) data containing all U.S. birth records from 2011-2016, I investigate changes in a variety of pregnancy-related outcomes which may be affected by the introduction of the ACA. I use simulated eligibility measures for the shares of women eligible for Medicaid and NMSI, following the methods developed in Currie and Gruber (1996a, 1996b, 2001). While many other ACA papers have used difference-in-difference (DD) or triple-difference (DDD) approaches to identify the impact of the ACA expansion (Charles Courtemanche et al., 2017; C. Courtemanche et al., 2016; Kaestner et al., 2017; Long et al., 2014), this paper is one of the first to use a simulated eligibility approach (Frean et al., 2017). Rather than use interactions between key policy aspects to generate an effect as with a DDD approach, using the(Frean et al., 2017) share eligible for policies allows me to capture the variation from ACA and subsidized health insurance plans prior to the ACA with greater accuracy while estimating their effects jointly.

Overall, I find the expansions in Medicaid and NMSI eligibility had little effect on pregnancy and birth related outcomes. However, I do find that NMSI eligibility lead to a reduction in the probability that a mother smokes cigarettes prior to pregnancy among all racial-ethnics groups except for non-Hispanic blacks. I also find an increase in the share of women eligible for NMSI who are not eligible for pregnancy-conditional Medicaid reduced the probability a birth was paid by Medicaid.

2.2 Background

This section describes pregnancy related aspects of health insurance prior to the ACA and then after. Focusing first on public and then on private insurance, I provide an overview regarding the evolution of these insurance types and their remaining shortcomings while reviewing the literature on how such insurance affected prenatal care, maternal health behaviors, and birth outcomes. I then highlight how the ACA changed these types of insurance in ways that affect access to maternity services.

2.2.1 Pregnancy-conditional Medicaid

Stark racial and socioeconomic birth disparities attracted attention in the 1980s, when the incidence of LBW was three times greater for black than white infants (Institute of Medicine, 1985; Kleinman & Kessel, 1987). During the 1970s and 1980s research on birth outcomes and disparities established a policy paradigm that increasing prenatal care could reduce the number of LBW infants and reduce the infant mortality rate (Alexander & Kotelchuck, 2001; Dave et al., 2018). Especially influential was the IOM's publication *Preventing Low Birthweight* in 1985. The study recognized that while prenatal care use and incidence of LBW were correlated, the relationship was not necessarily causal. Women who access prenatal care are generally of higher socioeconomic standing and consequently have better birth outcomes. The committee looked for causal evidence and came to the conclusion that studies "provide substantial evidence that high quality prenatal care begun early in pregnancy can lower the incidence of low birthweight" (Institute of Medicine, 1985, p. 132). The study found that blacks, younger women, and first-time mothers were groups that were likely to delay prenatal care, and that cost was a significant reason for this delay. The IOM also reviewed studies which showed Medicaid increased access to prenatal care. The committee concluded that expanding Medicaid should be a "part of a comprehensive program to reduce the nation's incidence of low birthweight" (Institute of Medicine, 1985, p. 156).

During the second half of the 1980s, Medicaid was expanded for low-income pregnant women through a series of policies. By 1989, states were required to cover all pregnant women up to 133%FPL and allowed to expand this coverage to higher income thresholds (DeLeire et al., 2011; Joyce et al., 1998). Currie and Gruber (1996b) use variation in state expansions of Medicaid for pregnant women during the 1980s to show how the Medicaid expansion affected pregnancy related outcomes.[1] They find that eligibility reduced delays in prenatal care and led to a small and marginally significant reduction in LBW. More significantly, expansion in eligibility also led to a sizeable reduction in infant mortality. The study suggests that the reduction in mortality, but not LBW, may mean improvements stemmed from better delivery and infant care rather than from better prenatal care.

[1] Currie and Gruber (1995) also examines the effect of this eligibility change, though the main focus is on physician fees.

Currie and Gruber (1996b) point out that a limitation of the expansion was low take-up, especially for higher income women. They determined the program was costly and ineffective for relatively higher income women. They suggest that among this group, women may not realize they are eligible and do not enroll until delivery, when the hospital is likely to enroll them. In this case, coverage does not increase preventative care through prenatal visits, but may increase expenditures during delivery. While they did not analyze birth-related procedures, Haas et al. (1993) found an increase in caesarean sections following the start of a Massachusetts insurance program for low-income, uninsured pregnant women in the mid-1980s.

Another paper examining the take-up issues that pregnancy-conditional Medicaid faced is Currie and Grogger (2002). Under the Aid to Families with Dependent Children (AFDC) program, families enrolled in AFDC were also automatically enrolled in Medicaid. When AFDC was reformed into Temporary Assistance for Needy Families (TANF) in 1996, women had to apply separately for Medicaid, which increased nonmonetary costs of applying. Currie and Grogger find higher welfare caseloads lead to more prenatal care use and lower infant mortality, arguably because when more women were using welfare they were more aware of their Medicaid eligibility. This suggests that lack of awareness of eligibility may be a barrier in states with lower welfare caseloads. States implemented various reforms to assist women with enrollment, such as mail-in-applications or out-stationing Medicaid eligibility workers in hospitals, but these measures were found to have insignificant effects. Another study analyzed Tennessee's implementation of presumptive Medicaid eligibility for pregnant women which issued temporary Medicaid cards for women likely eligible to use while they enrolled. Following this policy, mothers using Medicaid were 30% more likely to have a prenatal care visit in the first trimester, though there were no significant changes in birth outcomes (Piper et al., 1994).

Participation concerns also exist on the supply side, given Medicaid's low reimbursement rates to physicians. This issue has been studied broadly (T. C. Buchmueller et al., 2013; Cunningham & O'Malley, 2009; Decker, 2012; Sloan et al., 1978), and several studies have looked at maternity services specifically. Using data from the 1980s, higher Medicaid fees reduced infant mortality according to Currie et al. (1995) and increased birth weights according to Gray (2001). A later study from 2001-2010 found higher reimbursement rates increased prenatal care while producing some improvements in birth weights (Sonchak, 2015).

Overall, pregnancy-conditional Medicaid has created some improvement in prenatal care use and birth outcomes, but birth outcome disparities persist. The program has faced challenges with getting women to take up the insurance early on in their pregnancies, which means the benefits may stem more from delivery related procedures than preventative care.

2.2.2 Private Insurance and Maternity Services

In 2009, the year before the Affordable Care Act was passed, the majority (57.1% according to the American Community Survey (ACS)) of childless women 20-45 were insured through an Employer Sponsored Insurance (ESI) plan that came through their employer or the employer of their partner or parent (ACS data). [2]

Women without access to ESI could purchase non-group health insurance directly from an insurance company, but these plans were often quite expensive and the coverage could be fairly limited. Because people with health needs that cause them to foresee needing medical coverage are the most likely to purchase non-group insurance, the costs to cover these individuals were high. This in turn drives up premiums and further insures that only the most medically needy purchase non-group insurance, a phenomenon known as the plan death spiral (Cutler & Reber, 1998; Cutler

[2] Calculated using the insurance hierarchy approach based on Abraham et al. (2013).

& Zeckhauser, 1998). A common practice to keep costs down was for plans to refuse to cover services related to pre-existing conditions. Prior to the ACA, some states passed guaranteed issue laws to prohibit denying people coverage and some passed community rating laws to require insurers to charge the same price to everyone. Most studies found evidence of adverse selection in states with these programs as evidenced by lower enrollment of healthier people and higher premiums (Monheit et al., 2004; Sasso & Lurie, 2009; Simon, 2005); however a study on New York's community rating law did not find evidence of adverse selection (T. Buchmueller & DiNardo, 2002).

The issues with the non-group market were particularly problematic for women. According to one estimate, 92% of non-group plans in 2012 were gender-rated, meaning they charged women higher premiums for the same coverage (Garrett et al., 2012). Some states had laws to ban or limit gender rating, but the ACA ended this practice nationally (Garrett et al., 2012). Additionally, 62% of families purchasing non-group insurance were not covered for maternity care (Department of Health and Human Services, 2011). Women have a fair amount of control over pregnancy and those who planned to become pregnant could seek out insurance which would cover this health event. Given the large expenses and the asymmetric information involved with pregnancies, non-group insurers were incentivized not to cover maternity services. In states that did not require non-group plans to cover maternity care, only 6% of plans did (Garrett et al., 2012). If a woman with one of these plans became pregnant, the costs related to her pregnancy and delivery would have to be paid out of pocket. Insurance plans and riders which did cover maternity care typically had wait periods before they would be effective. Pregnancies which occurred before the wait periods ended would be deemed pre-existing conditions and not covered (Garrett et al., 2012).

While coverage for maternity services was sparse and expensive among non-group plans, such coverage has been standard for ESI plans. Since the Pregnancy Discrimination Act of 1978, ESI plans for employers with at least 15 employees have been required to provide maternity services for employees and spouses (Andrews, 2012; Norris, 2017). However, ESI plans have not been required to provide maternity services to dependents, and the majority have not (Andrews, 2012).

2.2.3 Affordable Care Act and Maternity Services

The ACA was an extensive overhaul to the U.S. health insurance system which had many different components to touch on a variety of issues, including some of the ones previously mentioned. Much has been written on the ACA expansion elsewhere, including in the first chapter of this dissertation, so here I will recap the basics of the ACA and highlight attributes particularly related to maternity services.

One key subsidized insurance expansion component of the ACA was to make Medicaid available to all adults at or and below 138%FPL. Prior to the ACA, Medicaid was available only to low-income adults who were pregnant, disabled, elderly, or parents. While the categorical eligibility groups and their respective income thresholds remained in place, the ACA moved Medicaid beyond categorical eligibility and made it possible for non-disabled, non-pregnant, childless women of childbearing age to be eligible. The Medicaid expansion was supposed to rollout nationally January 1, 2014 (with the exception of 5 early-expanding states and DC); however, because the Supreme Court made the Medicaid expansion optional, only 19 states expanded in January 2014. By April 2016, another 6 states had expanded, and the remaining 20 had not (KFF, 2017b). In expansion states, childless, non-pregnant women could qualify for Medicaid based solely on their income being 138%FPL or below. However, women living in non-expansion states remain ineligible for Medicaid if they do not meet the previously existing categorical Medicaid eligibility groups, including pregnancy-conditional Medicaid, which remained the same. Both before and after the ACA, women enrolled in Medicaid for pregnancy receive fully subsidized coverage for all care related to their pregnancy, delivery, and 60 days of postpartum care.

The other key insurance expansion in the ACA was the reform of the non-group insurance market. This involved developing the Marketplace where people could purchase non-group insurance plans. The ACA also provided subsidies to help people afford Marketplace plans. The subsidies are available for people within 100 to 400%FPL who do not have access to another health insurance plan which provides minimum essential coverage at an affordable price. The ACA banned gender-rating, which ended the previously common practice of charging women more than men.

Given that Medicaid counts as an affordable plan with minimum essential coverage, once a person is determined to be Medicaid eligible, they are no longer eligible for the premium tax credits or cost sharing reduction subsidies for Marketplace plans. They may remain on the Marketplace plan, but they will have to pay the full market price (Healthcare.gov, 2008). Thus, sliding-scale Marketplace subsidies are available for women between 139-400%FPL in expansion states and 100-400%FLP in non-expansion states. Additionally, some women with Marketplace plans prior to conception become eligible for Medicaid when pregnant, and therefore ineligible for Marketplace subsidies.[3] This group consists of those between 138%FPL and their state's Medicaid pregnancy income threshold in expansion states and women between 100%FPL and their state's Medicaid pregnancy income threshold in non-expansion states. Since the maximum pregnancy

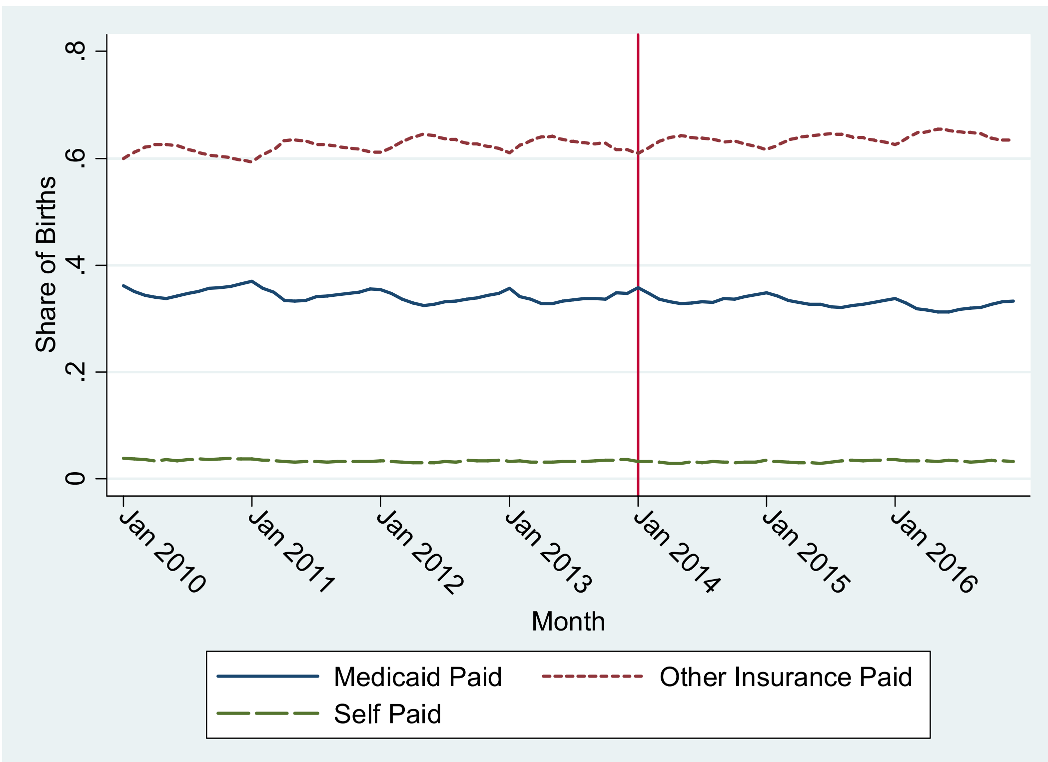
[3] The enforcement of this, however, remains unclear.

threshold ranges among states from 138 to 380%FPL, the size of this group differs greatly between states.

The cost of pregnancy remained unchanged for all the women in the newly Medicaid-eligible population since all states covered pregnant women up to at least 138%FPL prior to 2014. Women who are Marketplace eligible when not pregnant but Medicaid eligible when pregnant also did not experience a change in the cost of pregnancy since they should use Medicaid to pay for their pregnancies. However, women using Marketplace plans to pay for pregnancies experienced significant improvements in the insurance options available to them.

Prior to the ACA, women who lacked ESI and had incomes above the thresholds for Medicaid for pregnancy would struggle to find an insurance plan with affordable coverage for maternity services. Starting in January 2014, all non-group insurance plans are required to cover maternity and newborn care. Furthermore, the ACA prohibits private health plans, including Marketplace plans, from charging cost sharing for many preventative health services including prenatal care, tobacco cessation counseling and interventions, and breastfeeding services (KFF, 2015, 2016). The ACA prevents insurance companies from denying coverage or charging people more for pre-existing conditions, so even if a pregnancy occurred before the plan became effective, maternity services would still need to be provided. Marketplace coverage reduces the cost of prenatal care, delivery, and postpartum care.

While the ACA substantially improved insurance available to women, especially in areas related to maternity-services, it did not create a large expansion in insurance coverage for births at the extensive margin. Prior to the ACA very few births were self-paid despite moderately high rates of uninsurance among childbearing age women and limited coverage of maternity services in non-group plans. A large share of uninsured women could enroll in Medicaid when they became pregnant. A study of childbearing aged women from 2000 to 2009 found that 19% of non-pregnant women reported being currently uninsured as opposed to 10% of pregnant women (Kozhimannil et al., 2012). Among births to first time mothers in 2010 who were 20-45 years old and U.S. residents, only 3.7% were self-paid. Medicaid paid for 35.1% and other forms of insurance were used for the remainder (VS calculation). Figure 2.1 graphs the share of births paid by these three sources. It notable that the share of births that were self-paid has remained fairly stable despite the ACA.



Source: US natality records, the ACS, and the Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data for women 20-45 years old giving birth for the first time

Figure 2.1 Payment Sources for Births

2.2.4 *Influencing Birth Outcomes*

Much research has been done to attempt to explain the pervasive racial disparities in birth outcomes and to find policies to ameliorate these issues. Negative birth outcomes such as preterm and LBW infants are highly correlated and have both been used to measure adverse birth outcomes (Kramer & Hogue, 2009; Lauderdale, 2006). In 2016, the incidence of LBW was 13.68% among non-Hispanic blacks compared to 6.97% among non-Hispanic whites. Additionally, 13.77% of births among non-Hispanic blacks were preterm compared to 9.07% among non-Hispanic whites (Martin et al., 2018). Racial disparities exist even after controlling for socioeconomic variables (David & Collins Jr, 1997; McGrady et al., 1992; Schoendorf et al., 1992). Furthermore, some results indicate that higher socioeconomic variables correlate with better outcomes for whites but not blacks, making the racial disparities starker among higher socioeconomic women (Berg et al., 2001; P. A. Braveman et al., 2015; Starfield et al., 1991).

Timely and adequate prenatal care has been an important policy goal since the inception of pregnancy-conditional Medicaid. Yet, even if prenatal care improves, there is some ambiguity about whether it will improve birth outcomes. Associational studies have found that prenatal care use is associated with better birthweight and gestational length outcomes (Institute of Medicine, 1985; Murray & Bernfield, 1988; Tayebi et al., 2014), but women who seek out prenatal care may be more likely, based on socioeconomic factors, to have better birth outcomes holding prenatal care constant. Several studies have attempted more causal estimates related to prenatal care. Sonchak (2015) uses Medicaid reimbursement fees as an instrument variable (IV) for prenatal care. She finds a 10% increase in Medicaid reimbursement fees for obstetric care raises the number of prenatal care visits by between 0.06 and 0.11 visits depending on the demographic group, and that an additional visit coming from this exogenous difference increased birthweight by 24 grams for white mothers but had no effect for black mothers, suggesting this worsened disparities. Another IV uses the Port Authority Transit strike, which would have made getting to prenatal care appointments difficult for low-income mothers, to find prenatal care visits had little effect on birth weight (Evans & Lien, 2005). Currie and Gruber (1996b) find increased Medicaid eligibility improved prenatal care use and reduced infant mortality. Similarly, Gray (2001) found that higher Medicaid reimbursement fees reduced the incidence of LBW while and were associated with more prenatal care. However, rather than using these policies as an IV for prenatal care, they estimate prenatal care and infant mortality as separate outcomes and suggest that prenatal care is a probable causal pathway. There is some agreement that prenatal care may increase birth weight among full term births, though it may not be effective at preventing the incidence of preterm births (Conway & Deb, 2005; Currie & Grogger, 2002).

As previously mentioned, postnatal procedures may be more important in reducing infant mortality than prenatal care. Alexander and Kotelchuck (2001) review prenatal care studies to conclude that the reduction in child mortality has stemmed more from improvements in birth weight-specific mortality than improvements in birth weights. This suggests the improvements result from technology advances in post-birth care rather than increases in prenatal care.

The growing evidence that prenatal care may not be the correct channel to explain and improve birth outcome disparities has started to shift more focus to preconception health (Kramer & Hogue, 2009). While the 1985 IOM report called for "more emphasis on reducing risks associated with LBW before pregnancy occurs," this aspect of the report was mostly overlooked relative to the prenatal care recommendations (IOM, 1985, p. 150). To the degree which health insurance can improve health behaviors or health, preconception insurance could improve pregnancy outcomes. Healthy habits such as nutrition, exercise, and not smoking are helpful to establish before pregnancy. In terms of preconception health status, it is well established that underweight mothers have a higher risk of preterm and LBW infants (Cnattingius et al., 1998), but there is evidence they have lower risk of gestational diabetes, pre-eclampsia, and postpartum hemorrhage (Sebire et al., 2001). The health risks appear to be more severe for obese women (Kristensen et al., 2005), with one study finding twice the risk of stillborn and neonatal death among obese women relative to normal weight women (Kristensen et al., 2005).

Maternal stress is likely a strong determiner of poor birth outcomes, and may explain some of the racial and socioeconomic disparities in births which cannot be fully explained by income or socioeconomic differences. Several studies have suggest that stress from racial discrimination could play an important role (P. Braveman et al., 2017; Dominguez, 2008). Kramer and Hogue (2009) review a number of studies analyzing possible causal pathways for racial disparities including early-life stress and racism. The incident of LBW of African-born blacks more closely resembled U.S.-born whites than U.S.-born blacks—even after matching women by age, education, marital status, prenatal care, and history of fetal loss—suggesting more than genetic differences (David & Collins Jr, 1997). A study in California found that the incidence of LBW infants increased to mothers with Arabic-sounding names following the September 11th terrorist attack (Lauderdale, 2006). More broadly, factors like neighborhood effects (Matoba & Collins, 2017; Messer et al., 2006; Morenoff, 2003) and stressful life events (Graignic-Philippe et al., 2014; Sable & Wilkinson, 2000) have been associated with poor birth outcomes.

2.3 ACA's Potential Impacts

The ACA's extensive insurance system reforms targeted a number of the factors which impact birth outcomes. This section will lay out how the ACA has changed maternity services for women eligible for Medicaid or other subsidized insurance, and in turn make some predictions about how prenatal care, maternal health behaviors, delivery procedures, and birth outcomes may have been affected.

As mentioned earlier, pregnancy-conditional Medicaid was not changed by the ACA. For women below their states income threshold for pregnancy-conditional Medicaid, the ACA did not change the coverage or prices of their Medicaid insurance when pregnant. However, women using non-group, non-Medicaid plans to pay for their births saw improvements to coverage and prices since maternity services became mandatorily covered on non-group plans, preventative health care including prenatal care visits became free, and coverage could no longer be excluded because of pre-existing pregnancy conditions.

While coverage during pregnancy did not change for Medicaid eligible women, all women eligible for some type of subsidized insurance saw improvements in their coverage prior to pregnancy which could improve pregnancy outcomes. Compared to the past when pregnancy-conditional Medicaid beneficiaries might not have realized they were eligible, women are enrolled in Medicaid or a subsidized Marketplace plan prior to becoming pregnant know they have insurance. Furthermore, having pre-pregnancy insurance increases the probability of the woman knowing how and where she can schedule an appointment. Prior familiarity with medical institutions reduces the emotional and time costs of scheduling a first prenatal care visit. Awareness of pregnancy-conditional Medicaid eligibility was stressed as an issue around its inception in the late 1980s and early 1990s, and while awareness is presumably less problematic now it likely still exists.

Insurance expansions raise concerns about increasing *ex-ante* moral hazard. If women know insurance will help pay for any negative health outcomes, they have less incentive to invest in preventative health measures. Studying the 1980s pregnancy-related Medicaid expansions Dave et al. (2018) found evidence of moral hazard in terms of increased smoking and lowered weight gain during pregnancy. This moral hazard effect is likely to be even greater for ACA's Medicaid and NMSI plans because women can remain on the plans beyond 60 days postpartum when pregnancy-contingent Medicaid plans end.

Additionally, having long-term, subsidized insurance either through Medicaid or a non-Medicaid subsidized plan creates an income effect when subsidies reduce spending that otherwise would have been used on health care and can now be spent on other goods and services. How this income effect impacts health depends on whether the money is reallocated to health-improving or health-deteriorating expenditures.

Other determinants of birth outcomes which are less related to maternity services are preconception health and maternal stress. The ACA's expansion of preconception insurance may eventually improve preconception health and then impact birth outcomes; however, these are longer term impacts than cannot be addressed in this paper. Reducing maternal stress stemming from complex societal issues such as racism is largely beyond the sphere of insurance's influence. It is possible that the income effect from subsidized insurance may reduce stress to a small degree. Additionally, the point of insurance is not to simply pay for health expenditures but to smooth expenditures over risky health outcomes. Being insured against poor health outcomes has the potential to reduce some anxiety for the mother.

Another area of consideration is whether expanding subsidized insurance affects who decides to give birth. From the first chapter of this dissertation I point out that this expansion could reduce the share of unintended pregnancies both because of better access to affordable, effective forms of contraception and because women who wanted to have children but had delayed doing so due to cost considerations may choose to give birth. While Medicaid eligibility had no effect on the birth rate, NMSI eligibility led to significant increases in the birth rate. Intended pregnancies tend to have earlier prenatal care initiation and better birth outcomes (Brown & Eisenberg, 1995), so a higher share of intended pregnancies would improve these outcomes as a result of a change in the composition women giving birth. Grossman and Joyce (1990) develop a production function of infant health which addresses the selection of who decides to give birth, but this issue has not even mentioned in the literature let alone been accounted for empirically. Given the difficulties with evaluating how subsidized insurance affected maternity related outcomes using a sample-selection framework, this paper will not account for the shifts in who gives birth. This means estimates will be biased towards overestimating improvements in maternity-related outcomes since some of the effect could be stemming from changes in the composition of mothers rather than being relevant for the marginal women gaining access to subsidized insurance prior to conception.

With the exception of moral hazard and potentially the income effect, all of these features point towards improved maternity related outcomes. Awareness of being insured should lead to earlier prenatal care for women eligible for Medicaid and NMSI prior to conception unless the *ex-ante* moral hazard effect dominates and makes women apathetic to getting timely prenatal care.

Prenatal visits are designed to tell women what they should and should not do for healthy pregnancies. If earlier insurance coverage increases access to prenatal care and that advice is heeded, there should be less smoking and better nutrition during pregnancy. Improved nutrition should reduce the incidence of gestational diabetes and hypertension and improve appropriate weight gain during pregnancy. However, the findings of Dave et al. (2018) suggest the moral hazard aspects may dominate, leading to increased smoking and inadequate weight gain during pregnancy. The theoretical expectations are thus ambiguous. Additionally, if insurance improves preconception health, pre-pregnancy smoking and the body mass index (BMI) of women should improve. Charles Courtemanche and Zapata (2014) found the Massachusetts insurance reform in 2006 lead to statistically significant reductions in BMI but had insignificant effects on smoking.

Reductions in cost of maternity services for women gaining eligibility for NMSI may increase the use of more intensive birth procedures such as induced labor, caesarean sections, anesthesia, and neonatal intensive care unit (NICU) use. Finally, it is important to look at how increased eligibility for subsidized insurance effects the outcomes of the infants. Improvements in care before or during pregnancy should result in fewer preterm and LBW infants. On the other hand, more intensive delivery and post-natal care could reduce infant mortality, disproportionately improving the survival of preterm and LBW infants.

2.4 Methods

In order to assess how expanded subsidized insurance affected maternity outcomes, I create two measures: one for Medicaid eligibility and one for NMSI eligibility. The latter encompasses people eligible for Marketplace subsidies and some state-level subsidized insurance plans which existed prior to the ACA and provided partially subsidized insurance similar to subsidized Marketplace plans. A list of pre-ACA, state plans for childless adult plans is given in Table A.1. The list includes plans that did not have binding waitlists nor participating employer restrictions. This table also includes pre-ACA Medicaid plans for childless adults that were created through early ACA expansions or using special waivers. Those plans are denoted with an asterisk. My sample consists of childless women and thus both plans have eligibility based solely on income and not additional characteristics such as being pregnant or a parent which would have different income thresholds. Using these two measures of subsidized insurance focuses on the comprehensive benefits of gaining subsidized insurance rather than specific aspects of insurance which the ACA impacted.

My two measures of eligibility are generated using the simulated eligibility method developed by Currie and Gruber in several papers (Currie & Gruber, 1996b, 2001; Currie et al., 1995). Under this method, eligibility is the share of women from a national sample that meet the eligibility requirements in a given demographic group, state, and time period. Using a group level measure of eligibility rather than an individual's actual eligibility avoids issues of women changing their income in order to be eligible. Additionally, the birth data does not contain income information. The measure uses a national sample to abstract away from the demographics in the state, which may be correlated with policy decisions, and thus focuses on the effect of the policy generosity specifically. I follow the demographic groups laid out in Currie and Gruber (2001) and subsequently used by DeLeire et al. (2011) which are based on age, education, race, and—for women with at least a high school degree—marital status.[4]

In the simplest case of the ACA expansion, the eligibility for Medicaid variable in expansion states would go from 0% of women being eligible before January 2014 to the share of women nationally with incomes up to 138%FPL in January 2014. Eligibility for Medicaid in non-expansion states would start at 0% and remain there. However, because states expanded at different times and the income thresholds varied widely for Medicaid plans before January 2014, actual variation was more complex than the simplest case. Additionally, on the simplest level, Marketplace subsidies were available for women 100-400%FPL in non-expansion states and 139-400%FPL in expansion states starting January 2014. However, some states prior to the ACA had

[4] Age groups are 20-24, 25-34, and 35-45. Educational groups are no high school diploma/GED, high school diploma/GED, some college/AA degree, and bachelor's degree or higher. Racial groups are non-Hispanic white, non-Hispanic black, non-Hispanic other, and Hispanic. The marital categories are married and unmarried. See flow chart in Appendix for more detail.

state-level insurance programs for childless adults. Not accounting for these plans overstates the expansion of NMSI arising from the Marketplace.

For the simplest case of the Medicaid expansion, a DDD model could be used based on the idea that expansion states with larger populations below 138%FPL would experience greater impacts. The triple-interaction variable of expansion states, post ACA, and the share below 138%FPL would be the variable of interest and nearly identical to the simulated eligibility measure. The only difference is the triple interaction would measure share below 138%FPL using the sate sample while the simulated eligibility approach measures it from a national sample. Beyond the simplest case, the simulated eligibility approach allows for the eligibility measure to more accurately account for all the potential sources of variation. While I use simulated eligibility measures, I borrow from the DDD framework to inform the inclusion of additional control variables.

My model analyzes individual mother or infant level outcomes which reflect prenatal care use, maternal health behaviors, birth procedures, and birth outcomes. These variables will be described thoroughly in the next section. While the outcomes are measured at the individual level, eligibility is based on the demographic and state of the mother at the time of conception. The basic model is:

$$\begin{aligned} outcome_{ism} = \beta_0 + \beta_1 MedicaidElig_{gs(m\text{-}9)} + \beta_2 NonMedicaidElig_{gs(m\text{-}9)} \\ + \beta_3 Unemployment_{s(m\text{-}9)} + \beta_4 PostXExpander_{gsm} \\ + \beta_5 PostXBelow138_{gsm} + \delta_{gs} + \delta_{my} + \varepsilon_{ism} \end{aligned} \tag{1}$$

The share of women eligible for Medicaid and NMSI are represented by *MedicaidElig* and *NonMedicaidElig*, respectively. These share variables are continuous measures from 0 to 1, meaning the coefficients would be directly interpreted as a 100 percentage point change in eligibility. To be more reflective of the actual magnitude of the policy changes, I will scale these coefficients when discussing the results to reflect a 10 percentage point change in the share eligible. These eligibility measures are lagged using a standard 9 months for gestation to reflect the insurance eligibility at the time of conception. The *Unemployment* variable is the state's monthly unemployment rate at the time of conception. The model includes month-by-year and demographic group-by-state fixed effects. I use heteroscedasticity-robust standard errors clustered at the state level.

A DDD model allowing for more "bite" in areas with high shares below 138%FPL would start with DD indictors for the post period, whether a state is an expansion state, and the post period interacted with the expansion state indicator. It would also include the share below 138%FPL, the share below 138%FP interacted with the post period, the share below 139%FPL interacted with the expansion state indicator, and the share below 139%FPL interacted with the post period and the expansion state indicator. Of the DDD's control variables, I only include the interaction between the post period and state expansion status (*Post X Expander*) and the interaction between the post period and the share below 138% (*Post X Below138*) because these are the only controls not perfectly collinear with my model's fixed effects.

Given the large and well documented racial disparities in the use of prenatal care and birth outcomes, I stratify the analysis along the four racial groups used in creating the demographic groups: non-Hispanic white, non-Hispanic black, non-Hispanic other, and Hispanic.

2.4.1 Considering pregnancy-conditional Medicaid

Given the way subsidized insurance for non-pregnant women interacts with pregnancy-conditional Medicaid, I also run models with more complex measures of eligibility which account for the interactions between these different insurance programs. I calculate the share of women eligible for four different circumstances: 1). Always Medicaid Eligible 2). Medicaid Eligible when

Pregnant, Ineligible Otherwise 3). Medicaid Eligible when Pregnant, NMSI Eligible Otherwise 4). Always NMSI Eligible. The women who benefit from increased non-Medicaid subsidized eligibility and are ineligible for pregnancy-conditional Medicaid warrant particular attention since they are the group who experienced a decrease in the price of maternity related services. These finer eligibility measures allow me to specifically evaluate the share "Always NMSI Eligible."

Since the pregnancy-conditional Medicaid threshold is always higher than the general Medicaid threshold, and the upper limit for Marketplace subsidies is always higher than the pregnancy-conditional Medicaid threshold, these four eligibility groups capture progressively higher income groups of women. The "Always Medicaid Eligible" and "Medicaid Eligible when Pregnant, Ineligible Otherwise" groups can roughly be viewed as subgroups of the Medicaid Eligibility group in the main model. Similarly, the "Medicaid Eligible when Pregnant, NMSI Eligible Otherwise" and "Always NMSI Eligible" can be viewed as subgroups of non-Medicaid subsidized eligibility. For more detail, see Appendix A.1.

2.4.2 Robustness Checks

The methodology used in this paper avoids the endogeneity that may exist between an individual women's eligibility and her level of income since she can change her income in order to become eligible given her pregnancy expectations. However, there are concerns that policies may be based on, or correlated with, birth outcomes and could therefore be endogenous. If states which passed insurance expansions differed in pregnancy related outcomes from those which did not expand, non-expansion states would not serve as a valid counterfactual for expansion states in the post period making a DD approach invalid. The benefit of a DDD style approach is that it relies on weaker assumptions, specifically that accounting for differences in the share under 138%FPL in a state, the non-expansion states serve as a good representation of what the pregnancy outcomes would have been for expansion states had they not expanded, conditional on other controls. If the expansion had not occurred, then, conditional on other controls, the identifying assumption is that changes in pregnancy outcomes as well as the differential changes in pregnancy outcomes between expansion and non-expansion states must not be correlated with the share below 138%FPL. With respect to the Marketplace expansion, an additional identifying assumption is that changes in pregnancy outcomes would not be correlated with the share 139 to 400%FPL had the expansion not occurred, conditional on additional controls. While testing the counterfactual changes is impossible, testing for pre-expansion differences in these dimensions can help indicate if these assumptions seem plausible. Stated differently, the idea is that these thresholds take on new significance following the 2014 expansion because of the way they determine eligibility, but that aside from the eligibility policies, these variables would have no additional explanatory power beyond other controls.

The first test I use is an event study based on Charles Courtemanche et al. (2017) where I interact key variables related to the policy with each year, using 2013 as a base year. To test the Medicaid components of the expansion, I interact whether a state expanded, the share below 138%FPL, and the share below 138%FPL in expansion states with each year. Additionally, to account for income thresholds relevant to the Marketplace expansions, I interact the share between 139 and 400%FPL with each year. If the share below 138%FPL, the share below 138%FPL in expansion states, or the share between 139 and 400%FPL interacted with 2011 or 2012 are significant, it suggests that outcomes differed according to these dimensions even before the reform, which raises concerns about the validity of the identifying assumptions.

The event study tests the DDD framework which underlies my methodology, but the event study does not incorporate my specific eligibility measures. Thus I present another test which tests the share eligible measures. This placebo style test moves the expansion earlier by 6, 12, or 18 months and then uses eligibility measures and interaction terms on the pre-expansion data (conceptions during 2011-2013). The idea behind this test is that since those policies did not actually exist at those times, they should not affect outcomes. If they do, outcomes may have been changing in ways that are correlated with the eligibility expansion before it occurred. Unfortunately, this test is not perfectly clean since there were some subsidized insurance plans for childless adults prior to January 2014.

2.5 Data

This analysis relies on two main data sources: the American Community Survey (ACS) and the natality records component of Vital Statistics (VS) from the National Center for Health Statistics. I use data on births conceived from 2011 to 2016. Starting with 2011 conceptions helps avoid issues which could arise from the young adult expansion which took place in 2010. The young adult provision of the ACA allowed young adults to remain on their parent's insurance until the month they turn 26 and led to a sizeable increase in coverage (Barbaresco et al., 2015; Cantor et al., 2012; Sommers et al., 2012), but not one resulting from increases in subsidized insurance eligibility. Since low-income teens had Medicaid eligibility prior to the ACA, and because the age at conception is ambiguous for 19 year olds, my sample consists of women aged 20-45. I uses all the states and the District of Columbia except Connecticut and Rhode Island (due to missing maternal education data in the VS data) and Massachusetts and California (due to the nature of their early health care reforms).[5]

[5] The Massachusetts health care reform, which was a model for the ACA, took place in 2006. California was an early expansion Medicaid state, but rolled out its expansion at the county level making it difficult to align with the state level analysis here.

2.5.1 *Eligibility and Control Variables*

I use the ACS to construct the shares of women eligible for subsidized insurance. This is an annual survey of approximately 1% of the US population. Using monthly information about state policies and annual, national ACS data, I construct the variables for the share of women eligible for Medicaid and NMSI at the demographic group-state-month level. To find details on state-level policies I started with mostly annual information from the Kaiser Family Foundation (Dorn et al., 2004; 2017; 2010, 2017a) and then found specific dates of policy changes using information from Centers for Medicare and Medicaid Services or state websites. Constructing these measures using the national sample of the women in the ACS allows for better accuracy from more observations and removes the influence of state demographics which could be correlated with policy decisions.

The information on the states' pre-ACA plans and Medicaid expansion decisions is then combined with information on the states' pregnancy-conditional Medicaid policies to create the more detailed eligibility measures. I again use the national sample of women to calculate for each demographic group and state in each month the share of women who are: 1). Always Medicaid Eligible 2). Medicaid Eligible when Pregnant, Ineligible Otherwise 3). Medicaid Eligible when Pregnant, NMSI Eligible Otherwise 4). Always NMSI Eligible.

For example, Nevada had no pre-ACA insurance programs for childless adults, expanded Medicaid effective January 1st, 2014, and has had its pregnancy-conditional Medicaid income threshold at 165%FPL. Prior to its January 2014 expansion, no Nevadan women would fit the first category, women 0-165%FPL would fit the second, and no women would fit into the third or fourth categories. After the expansion, women 0 -138%FPL would fit the first category, no women would be in the second, women 139-165%FPL fit the third category, and women 166-400%FPL would be in the fourth category. Calculating the share of the national sample which fits these thresholds during these time periods defines the eligibility measures for Nevada. Appendix A.1 provides further explanation.

I also used the ACS data to construct the *PostXBelow138%* variable, which is one of the DDD inspired controls. These "bite" variables in DDD approaches are usually fixed measures based on the pre-period, but given some small demographic-state groups in the ACS, and the belief that the share below 138%FPL should not change significantly as a result of the policy, I chose to use data from 2011-2016 to construct this measure with greater accuracy. The other DDD inspired control, *PostXExpansion*, interacts post 2014 with a variable for whether the state expanded Medicaid, regardless of whether it was as an early, on-time, or late expansion. I also include the state monthly unemployment rate which comes from the Bureau of Labor Statistics.

2.5.2 Pregnancy and Birth Outcomes from Vital Statistics

The pregnancy related outcomes all come for the VS data. This data comprises the census of all U.S. births and includes detailed, individual-level information about demographics, health care utilization, pregnancy outcomes, and the health of the mother and infant. I choose outcomes which provide a fairly comprehensive look at health care utilization, behaviors, and outcomes, which I divide between those related to pregnancy and those related to delivery or birth outcomes.

The demographics allow me to assign women to the race, age, education, and marital groups which correspond to the ones used in the eligibility measures. Additionally, the restricted access version allows me to identify the mother's state of residency. I use women who are U.S. residents since Marketplace plans and its subsidies are available to citizens as well as non-citizens who are lawfully present (Siskin & Lunder, 2016). I also restrict the sample to births which are the mother's first live birth because there are different eligibility requirements for parents and because this creates a more homogenous sample without needing to account for birth parity.

The first pregnancy related outcome addressed is the timeliness of prenatal care. A standard measure to evaluate the timeliness of prenatal care is the share of women initiating prenatal care in the first trimester (Health Resources and Services Administration, 2011). Extrapolating from that to the individual level, I create a binary variable for whether the mother initiated prenatal care in the first trimester.

For pregnancy related behaviors, I focus on smoking and adequate weight gain. Smoking is known to be associated with elevated risks for a variety of pregnancy complications and pregnant women are highly encouraged to quit smoking during pregnancy (Castles et al., 1999). The VS data contains self-reported information on the number of cigarettes smoked daily at four intervals—prior to pregnancy and during each of the three trimesters. The data is likely downwards biased due to stigma effects. While the smoking measures in the VS data have been found to underestimate cigarette use relative to another survey of mothers who recently gave birth (Tong et al., 2013), the reporting is generally accepted to provide a fairly accurate representation of cigarette use (Nielsen et al., 2014). I create binary outcomes for cigarette use prior to pregnancy and third trimester cigarette use. When looking at third trimester cigarette use, I do not condition on smoking prior to pregnancy to avoid changes in the sample from women quitting prior to pregnancy.

During pregnancy, a woman should gain enough weight to support healthy development of her infant, but too much weight gain poses health risks for the mother and baby and makes post-pregnancy weight loss more difficult (Rasmussen & Yaktine, 2010). The IOM's maternal weight gain recommendations balance these risks by recommending different amounts of weight gain based on a mother's pre-pregnancy BMI. Relative to these recommendations, weight gain is classified as inadequate, adequate, or excessive (Rasmussen & Yaktine, 2010). I follow this standard definition by making binary outcomes for each of the three classifications of weight gain.

2.5.3 Birth variables

Given the increased coverage of maternity services among non-group plans, I investigate if delivery and post birth procedures increased using several binary indicators. With respect to delivery, I look at whether there was an increase in the probability that the birth used anesthesia or was delivered via a cesarean section. I choose these two delivery procedures because the decisions related to their use differ. While the mother may have more decision power over the use of anesthesia, the physician has more control over the decision to do a cesarean section. Additionally, both can be expensive components of birth related expenditures. Additionally, following birth, I test if there was an increase in neonatal intensive care unit use. Finally, an important post-birth outcome is whether the baby is breastfeeding. I use the VS data's indicator for whether the infant is being breastfed when it leaves the hospital.

When analyzing infant health, both LBW and preterm births have been used as indicators of poor birth outcomes given the advantages and disadvantages of each (Kramer & Hogue, 2009; Lauderdale, 2006; Wilcox, 2001). Birth weight is perhaps the most commonly used outcome and is more accurately reported than gestational weight. Misclassification of gestational age is particularly problematic since it tends to have more missing or unreasonable values among poor and minority women and LBW infants (Dietz et al., 2007; Lauderdale, 2006). However, LBW has been criticized for mixing together two distributions which represent two different scenarios: preterm birth and growth-retarded children (Conway & Deb, 2005; Wilcox, 2001). One solution has been to analyze birth weight outcomes using a mixture model (Conway & Deb, 2005; Lauderdale, 2006; Wilcox, 2001); however, for simplicity, this paper will look at both birth weight and gestational age separately. I create a binary variable for LBW based on whether the infant is under 2500 grams and a binary variable for preterm based on whether the infant is less than 37 weeks gestational age.

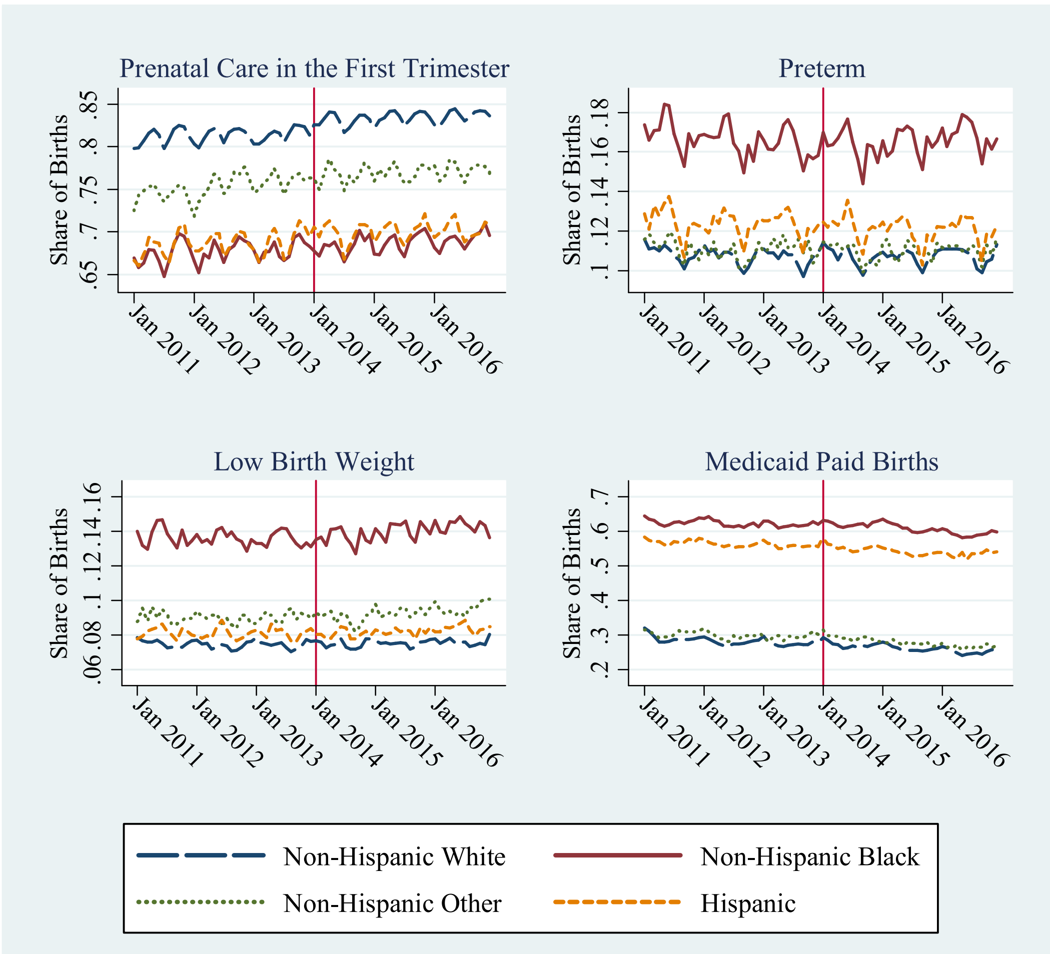
Table 2.1 Summary Statistics

		Non-Hispanic White	Non-Hispanic Black	Non-Hispanic Other	Hispanic
Prenatal in 1st Trimester	Pre	0.827	0.704	0.762	0.720
	Post	0.843	0.714	0.777	0.727
Pre-Pregnancy Smoker	Pre	0.150	0.077	0.041	0.043
	Post	0.125	0.065	0.033	0.036
3rd Trimester Smoker	Pre	0.075	0.032	0.017	0.013
	Post	0.062	0.027	0.013	0.010
Inadequate Weight Gain	Pre	0.154	0.210	0.234	0.194
	Post	0.159	0.211	0.245	0.201
Adequate Weight Gain	Pre	0.292	0.264	0.372	0.310
	Post	0.292	0.269	0.367	0.312
Excessive Weight Gain	Pre	0.553	0.526	0.394	0.496
	Post	0.549	0.520	0.388	0.487
Gestational Diabetes	Pre	0.048	0.045	0.091	0.050
	Post	0.051	0.046	0.107	0.054
Gestational Hypertension	Pre	0.075	0.080	0.040	0.057
	Post	0.087	0.094	0.052	0.068
Medicaid Paid Birth	Pre	0.253	0.598	0.296	0.522
	Post	0.236	0.577	0.264	0.499
Anesthesia	Pre	0.808	0.811	0.805	0.793
	Post	0.818	0.813	0.825	0.793
Cesarean Section	Pre	0.318	0.373	0.329	0.343
	Post	0.310	0.365	0.326	0.324
NICU	Pre	0.086	0.120	0.097	0.095
	Post	0.093	0.128	0.106	0.103
Breastfeeding	Pre	0.859	0.733	0.889	0.892
	Post	0.879	0.766	0.917	0.910
LBWT	Pre	0.074	0.137	0.090	0.081
	Post	0.075	0.142	0.094	0.082
Preterm	Pre	0.106	0.161	0.108	0.120
	Post	0.106	0.163	0.109	0.116

Source: VS data for women 20-45 who are giving birth for the first time. “Pre” corresponds to birth conceived during 2011-2013 and “Post” corresponds with 2014-2016.

2.5.4 Summary Statistics and Racial Disparities

As mentioned in the methods section, stark racial disparities exist among health care utilization during pregnancy and birth outcomes. In this section I present summary statistics for my outcome variables pre and post 2014 by racial and ethnic groups in Table 2.1.



Source: VS data for women 20-45 years old giving birth for the first time

Figure 2.2 Pregnancy Related Outcomes by Race

To further illustrate these disparities and to show how key measures have progressed over time, I provide the set of graphs in Figure 2.2. These graphs of women in the sample shows outcomes based on the month when the mother gave birth and contain no controls other than stratifying by race. Going clockwise from the upper left, the first graph shows the share of women with a prenatal care visit in the first trimester. Non-Hispanic whites clearly have the highest share of timely prenatal care, followed by the non-Hispanic other racial group, and then by Hispanics and non-Hispanic blacks. Timely prenatal care use is increasing across all racial groups with no clear indication of racial gaps in use narrowing. The next two graphs show the high rate of preterm and LBW births among non-Hispanic black mothers, especially compared to the much lower incidence of these outcomes among non-Hispanic Whites. While Hispanics have lower LBW incidence than the non-Hispanic other group, Hispanics have higher incidence of preterm births than the non-Hispanic other group. The final graph indicates the racial disparities between the share of births that are Medicaid paid, which largely reflects the racial differences in poverty.

2.6 Results

The effects of Medicaid and non-Medicaid subsidized eligibility on pregnancy related outcomes are shown in Table 2.2. Starting with the results in column 1, it appears that neither Medicaid nor non-Medicaid subsidized eligibility had any effect on the probability of a woman receiving prenatal care in the first trimester. With the exception of the non-Hispanic other group, the coefficients are generally positive, indicating a higher likelihood of timely prenatal care. However, eligibility for the non-Hispanic other group indicates negative effects on the probability of timely prenatal care, and the result is marginally significant for Medicaid eligibility. Yet, while statistically significant, this effect is hardly meaningful since it indicates that a 10 percentage point increase in Medicaid eligibility would lead to a 0.57 percentage point decrease in the probability of a woman having a prenatal care visit in the first trimester. Relative to the baseline mean of 76.2% for the non-Hispanic other group, this suggests a 0.7% change. The percent changes in timely prenatal care are similarly negligible for the other racial and ethnic groups.

The next two columns indicate improvements in maternal smoking behaviors. Column 2 shows the probability a mother smoked cigarettes prior to her pregnancy. Fairly consistently across the racial-ethnic groups, NMSI eligibility decreased the probability of pre-pregnancy cigarette use, and the coefficients are significant for all but the non-Hispanic black group. Additionally, these effect sizes are fairly large. A 10 percentage point increase in non-Medicaid subsidized eligibility decreases the probability a mother smoked prior to pregnancy by 0.39 percentage points for non-Hispanic whites, 0.24 percentage points for non-Hispanic other, and 0.12 percentage points for Hispanics, which correspond to a 2.6%, 5.9%, and 2.7% decreases among these groups when compared to the pre-period means.[6] The Medicaid eligibility coefficients are also negative, but insignificant.

The next column examines the probability that the mother used cigarettes during the third trimester. The coefficients are consistently negative, but the only non-Medicaid eligibility coefficient that is significant is for non-Hispanic whites. For non-Hispanic whites a 10 percentage point increase in NMSI eligibility reduced the probability of smoking in the third trimester by 0.15 percentage points, which corresponds to a 2% reduction. There is also some indication that Medicaid eligibility may reduce third trimester cigarette use among non-Hispanic blacks.

The remaining columns in Table 2.2 look at nutrition related factors of pregnancy. Columns 4-6 address whether maternal weight gain was inadequate, adequate, or excessive according to clinical standards that compare weight gain with pre-pregnancy BMI. Overall, there is no indication of substantial changes in the probability that a mother is included in one of these categories as a result of changes in subsidized insurance eligibility. The coefficients are occasionally marginally significant but never very large relative to the baseline means. However,

[6] These are calculated by dividing the percentage point changes by the baseline means. For example, the baseline mean for pre-pregnancy smoking among non-Hispanic whites was 15%, and 0.39/15=.026.

Table 2.2 Medicaid and Non-Medicaid Subsidized Eligibility on Pregnancy Outcomes

	Prenatal in 1st Trimester	Pre-Pregnancy Smoker	3rd Trimester Smoker	Inadequate Weight Gain	Adequate Weight Gain	Excessive Weight Gain	Gestational Diabetes	Gestational Hyper-tension
Non-Hispanic White								
Medicaid Eligibility	0.00809	-0.0162	-0.0107	-0.00556	-0.004	0.00957	0.000256	-0.0113
	(0.014)	(0.018)	(0.009)	(0.007)	(0.006)	(0.011)	(0.005)	(0.007)
NMSI Eligibility	-0.000248	-0.0388***	-0.0150**	0.00608	0.00519	-0.0113*	-0.0015	0.00599
	(0.004)	(0.010)	(0.005)	(0.004)	(0.004)	(0.005)	(0.002)	(0.004)
Unemployment	0.00384**	0.000333	0.00122*	-0.000833	0.000528	0.000306	0.000691	-0.000469
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Post X Expander	0.0246*	-0.0452***	-0.0585***	-0.000872	-0.00668	0.00755	-0.00445	-0.0042
	(0.009)	(0.013)	(0.007)	(0.005)	(0.005)	(0.008)	(0.003)	(0.004)
Post X Below 138%	0.00784	0.0039	0.00108	0.00092	-0.000684	-0.000236	0.00246	0.00749*
	(0.006)	(0.003)	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)
N=	3,164,486	3,174,264	3,173,906	3,140,714	3,140,714	3,140,714	3,249,134	3,249,134
Non-Hispanic Black								
Medicaid Eligibility	0.0188	-0.0103	-0.0119*	0.0182	-0.0041	-0.0141	-0.0119*	-0.0231
	(0.027)	(0.010)	(0.006)	(0.015)	(0.014)	(0.011)	(0.005)	(0.023)
NMSI Eligibility	0.0294	0.0041	0.00476	-0.00945	-0.0202	0.0297*	-0.00509	0.00558
	(0.023)	(0.007)	(0.004)	(0.013)	(0.017)	(0.012)	(0.004)	(0.008)
Unemployment	0.00757***	-0.000174	-0.000161	0.000378	-0.00218	0.0018	0.00180*	-0.0000666
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Post X Expander	0.0618*	-0.0203**	-0.0168***	-0.0112	0.0144	-0.00316	-0.00235	0.00375
	(0.025)	(0.007)	(0.004)	(0.019)	(0.017)	(0.008)	(0.005)	(0.006)
Post X Below 138%	0.00847	0.00306	0.00319*	-0.00298	-0.00679	0.00977	0.00619*	0.0222*
	(0.008)	(0.003)	(0.001)	(0.005)	(0.004)	(0.006)	(0.003)	(0.011)
N=	666,265	682,463	682,383	663,890	663,890	663,890	699,539	699,539
Non-Hispanic Other								
Medicaid Eligibility	-0.0569*	-0.00382	-0.0124	0.00945	0.00151	-0.011	-0.0378	-0.0204
	(0.028)	(0.016)	(0.012)	(0.019)	(0.013)	(0.024)	(0.027)	(0.016)
NMSI Eligibility	-0.00204	-0.0243*	-0.0112	-0.0124	0.00505	0.00733	-0.00459	-0.00633
	(0.015)	(0.009)	(0.006)	(0.014)	(0.014)	(0.014)	(0.009)	(0.008)
Unemployment	0.00391*	-0.000641	-0.000567	-0.000715	-0.00185	0.00256	0.00011	-0.000462
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
Post X Expander	0.0369**	-0.0151	-0.00882	-0.0229	0.0392***	-0.0163	0.00917	0.0169*
	(0.013)	(0.009)	(0.007)	(0.011)	(0.011)	(0.012)	(0.017)	(0.008)
Post X Below 138%	0.0216**	-0.000329	0.000532	0.00102	-0.00444	0.00341	0.0190**	0.0138**
	(0.007)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)	(0.007)	(0.004)
N=	367,996	373,528	373,458	367,265	367,265	367,265	382,092	382,092
Hispanic								
Medicaid Eligibility	0.00749	-0.00493	-0.00353	0.00582	-0.016	0.0102	-0.0121	-0.0374
	(0.034)	(0.012)	(0.005)	(0.014)	(0.026)	(0.032)	(0.007)	(0.023)
NMSI Eligibility	0.0212	-0.0116*	-0.00525	0.00207	-0.0258*	0.0237*	-0.0126***	-0.00272
	(0.017)	(0.005)	(0.003)	(0.009)	(0.011)	(0.009)	(0.003)	(0.005)
Unemployment	0.00424	0.000143	0.000123	-0.000796	-0.00208	0.00287	0.0000703	0.00169
	(0.003)	(0.001)	(0.000)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
Post X Expander	0.0295*	-0.0000422	-0.00355*	-0.00839	0.0216	-0.0132	-0.00915	0.00517
	(0.013)	(0.004)	(0.002)	(0.005)	(0.031)	(0.033)	(0.008)	(0.015)
Post X Below 138%	0.00876	0.00095	-0.00176	-0.00573	-0.0109	0.0166	0.00807**	0.0298**
	(0.009)	(0.003)	(0.001)	(0.005)	(0.008)	(0.012)	(0.002)	(0.009)
N=	761,905	785,636	785,666	765,711	765,711	765,711	792,336	792,336

Notes: Sample consists of women aged 20-45 giving birth for the first time. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. *p < .05, ** p < .01, *** p < .001

there appears to be some consistent decreases in the probability a mother has gestational diabetes (column 7) for both types of insurance eligibility and across racial-ethnic groups, though these effects are rarely significant. One case where the results are strongly significant and large is for the effect of NMSI eligibility on the probability of gestational diabetes for Hispanic women where a 10 percentage point increase in eligibility decreases the probability a woman has gestational diabetes by 2.4%. There are no significant effects of the eligibility measures on gestational hypertension (column 8).

Table 2.3 shifts attention to delivery and post birth outcomes. The first column tests whether these eligibility measures had any effect on the probability that a birth was Medicaid paid and finds no significant effects. Additionally, there seem to be no significant changes in the intensity of delivery related services such as anesthesia or cesarean sections (columns 2 and 3).

The two birth outcome measures, the probability the infant is LBW or preterm, indicate some significant changes as a result of the eligibility expansions, but the result seem to lack a consistent pattern across racial-ethnic groups. For example, a 10 percentage point increase in non-Medicaid subsidized eligibility leads to a 0.96% increase in the probability an infant is LBW among non-Hispanic whites but a 1.09% reduction in the probability among Hispanics.[7] This would suggest non-Medicaid eligibility subsidized eligibility reduces the LBW disparities, due to worsening outcomes for whites as well as improving outcomes for Hispanics. Additionally, for preterm births (column 5), the coefficients indicate a increases in the probability of preterm births for non-Hispanic whites and blacks, with a marginally significant increase for Medicaid eligibility among non-Hispanic blacks On the other hand, there were significant reductions in the probability of preterm births for non-Hispanic other and Hispanics from an increase in NMSI eligibility. There

[7] For non-Hispanic whites, 0.0712/7.4= 0.00962. For Hispanics 0.00882/8.1=.01088.

Table 2.3 Medicaid and Non-Medicaid Subsidized Eligibility on Delivery Outcomes

	Medicaid Paid Birth	Anesthesia	Cesarean Section	LBW	Preterm	NICU	Breastfeeding
Non-Hispanic White							
Medicaid Eligibility	-0.0666	-0.0171	0.00526	0.00409	0.00325	-0.0086	-0.00778
	(0.040)	(0.025)	(0.009)	(0.003)	(0.004)	(0.007)	(0.014)
NMSI Eligibility	-0.0263	0.00144	0.00631	0.00712**	0.00486	0.00759*	0.00564
	(0.017)	(0.016)	(0.003)	(0.002)	(0.003)	(0.004)	(0.010)
Unemployment	-0.000706	0.00342	0.00169	-0.000526	0.0000857	-0.000728	0.000398
	(0.002)	(0.003)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
Post X Expander	-0.015	-0.0159	-0.00494	0.003	0.00869*	0.00689	0.0566***
	(0.022)	(0.015)	(0.006)	(0.003)	(0.004)	(0.004)	(0.009)
Post X Below 138%	0.0228**	-0.0036	0.00367	-0.0000841	-0.000577	0.000529	-0.0022
	(0.008)	(0.007)	(0.002)	(0.001)	(0.001)	(0.002)	(0.005)
N=	3,229,760	3,250,799	3,251,405	3,252,863	3,010,514	3,245,957	3,208,427
Non-Hispanic Black							
Medicaid Eligibility	-0.0226	0.000741	0.000799	0.00792	0.0166*	-0.0182	0.002
	(0.049)	(0.027)	(0.011)	(0.006)	(0.007)	(0.011)	(0.023)
NMSI Eligibility	-0.0582	0.0117	-0.00551	0.0106	0.0129	0.000801	0.00987
	(0.030)	(0.015)	(0.011)	(0.010)	(0.008)	(0.009)	(0.020)
Unemployment	0.0012	0.00149	0.00274	-0.000887	-0.000906	-0.00166	-0.00511
	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
Post X Expander	0.0211	0.0106	0.0109	0.00315	-0.00159	-0.00288	0.0758***
	(0.020)	(0.013)	(0.012)	(0.005)	(0.007)	(0.006)	(0.011)
Post X Below 138%	0.0337*	0.00357	0.00239	-0.00398*	-0.00262	0.00452	-0.0178*
	(0.013)	(0.010)	(0.005)	(0.002)	(0.002)	(0.003)	(0.008)
N=	695,622	700,270	700,403	700,758	636,512	699,106	688,020
Non-Hispanic Other							
Medicaid Eligibility	-0.0574	-0.0569	-0.00159	-0.0179*	-0.00182	-0.0438	-0.0739*
	(0.062)	(0.049)	(0.019)	(0.008)	(0.010)	(0.024)	(0.032)
NMSI Eligibility	0.00895	0.0284	0.0137	-0.00616	-0.0213**	0.00957	0.0389**
	(0.022)	(0.037)	(0.014)	(0.006)	(0.008)	(0.010)	(0.013)
Unemployment	0.00328	0.00534	0.00247	-0.00145	0.00289*	-0.000501	-0.00257
	(0.002)	(0.005)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)
Post X Expander	0.00106	0.0152	0.00825	0.0169*	0.0176*	0.0254*	0.0757**
	(0.032)	(0.027)	(0.014)	(0.008)	(0.008)	(0.012)	(0.025)
Post X Below 138%	0.0336**	0.0238	0.00809	0.00371	0.00254	0.0118	0.0126
	(0.010)	(0.018)	(0.006)	(0.002)	(0.003)	(0.007)	(0.009)
N=	379,322	382,234	382,334	382,478	351,147	381,668	377,344
Hispanic							
Medicaid Eligibility	-0.0877	0.0301	0.0315	0.00162	0.0108	-0.0172	-0.0242
	(0.052)	(0.041)	(0.017)	(0.003)	(0.008)	(0.012)	(0.015)
NMSI Eligibility	-0.0531	-0.0139	0.0122	-0.00882*	-0.0152*	-0.0114**	0.0199**
	(0.029)	(0.026)	(0.008)	(0.004)	(0.007)	(0.004)	(0.007)
Unemployment	0.00333	0.00502	0.00351*	0.000447	0.00207	0.000696	-0.000911
	(0.004)	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Post X Expander	0.0276	-0.0628	-0.0243	-0.000914	-0.0134	-0.0029	0.0332**
	(0.016)	(0.058)	(0.022)	(0.004)	(0.009)	(0.009)	(0.011)
Post X Below 138%	0.023	0.0197*	0.0162**	0.0015	0.00219	0.00593	-0.00643
	(0.014)	(0.009)	(0.006)	(0.001)	(0.002)	(0.004)	(0.004)
N=	784,791	792,728	792,739	793,008	727,906	792,053	786,073

Notes: Sample consists of women aged 20-45 giving birth for the first time. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

results suggest subsidized insurance eligibility hurt birth outcomes for non-Hispanic whites and blacks while having more positive effects for the non-Hispanic other and Hispanic groups.

Change in the probability of NICU use (column 6) as a result of changes in eligibility seem to mirror how eligibility affected the probability of an infant being LBW (column 4). Among non-Hispanic whites, a higher share of women eligible for NMSI led to a higher probability of NICU use, which is consistent with finding this eligibility increased the probability of LBW infants. On the other hand, Hispanics experienced a decrease in LBW and NICU use when the share eligible for NMSI increases.

Higher shares of women eligible for NMSI has a consistently positive effect on the probability of the infant breastfeeding when leaving the hospital, and this result is significant for non-Hispanic other and Hispanics. However, given the high baseline means, the magnitude of this effect is small. The 0.389 and 0.199 percentage point increases in the probability of breastfeeding for non-Hispanic Other and Hispanics from a 10 percentage point increase in NSMI corresponds to a 0.44% and 0.22% increase respectively.

2.6.1 Results Considering Policy Interactions

In order to hone in on the effects of increasing subsidized insurance eligibility for women who are not eligibility for pregnancy-conditional Medicaid, I use detailed eligibility variables that reflect the interaction between subsidized insurance eligibility for non-pregnant women and pregnancy-conditional Medicaid. Table 2.4 presents the pregnancy related results and Table 2.5 presents the birth related ones. The coefficients of unemployment, post interacted with expansion state, and post interacted with the share below 138 are suppressed from Table 2.4 and Table 2.5, but still included in the regressions.

Table 2.4 Detailed Eligibility Interactions on Pregnancy Outcomes

	Prenatal in 1st Trimester	Pre-Pregnancy Smoker	3rd Trimester Smoker	Inadequate Weight Gain	Adequate Weight Gain	Excessive Weight Gain	Gestational Diabetes	Gestational Hypertension
Non-Hispanic White								
Always Medicaid Eligible	0.0184	0.0021	0.0190	-0.0253	-0.0161	0.0413*	0.0051	-0.0101
	(0.034)	(0.018)	(0.011)	(0.014)	(0.012)	(0.017)	(0.007)	(0.013)
Medicaid when Pregnant, Ineligible Otherwise	0.0092	0.0290	0.0386**	-0.0204	-0.0121	0.0325	0.0069	0.0024
	(0.032)	(0.018)	(0.011)	(0.015)	(0.014)	(0.018)	(0.006)	(0.015)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.0199	-0.0448*	-0.0010	-0.0222	-0.0134	0.0355*	-0.0019	0.0019
	(0.036)	(0.019)	(0.012)	(0.016)	(0.013)	(0.016)	(0.006)	(0.014)
Always NMSI Eligible	-0.0073	-0.0107	0.0057	0.0095	0.0061	-0.0156	0.0043	0.0107
	(0.008)	(0.010)	(0.006)	(0.006)	(0.006)	(0.008)	(0.004)	(0.007)
N=	3,164,486	3,174,264	3,173,906	3,140,714	3,140,714	3,140,714	3,249,134	3,249,134
Non-Hispanic Black								
Always Medicaid Eligible	-0.0334	-0.0193	-0.0144	0.0507	-0.0067	-0.0441	-0.0019	-0.0659*
	(0.034)	(0.016)	(0.009)	(0.029)	(0.023)	(0.030)	(0.009)	(0.026)
Medicaid when Pregnant, Ineligible Otherwise	-0.0627	-0.0077	-0.0031	0.0394	-0.0039	-0.0355	0.0110	-0.0479**
	(0.032)	(0.014)	(0.007)	(0.025)	(0.019)	(0.029)	(0.011)	(0.016)
Medicaid when Pregnant, NMSI Eligible Otherwise	-0.0014	-0.0133	0.0032	0.0091	-0.0185	0.0093	0.0047	-0.0324
	(0.027)	(0.015)	(0.008)	(0.025)	(0.024)	(0.029)	(0.009)	(0.017)
Always NMSI Eligible	-0.0144	0.0157	0.0047	0.0184	-0.0283	0.0098	-0.0020	-0.0063
	(0.025)	(0.010)	(0.005)	(0.026)	(0.025)	(0.015)	(0.010)	(0.014)
N=	666,265	682,463	682,383	663,890	663,890	663,890	699,539	699,539
Non-Hispanic Other								
Always Medicaid Eligible	-0.0333	0.0068	0.0139	-0.0168	-0.0117	0.0284	-0.0530	-0.0193
	(0.051)	(0.033)	(0.020)	(0.028)	(0.029)	(0.034)	(0.033)	(0.017)
Medicaid when Pregnant, Ineligible Otherwise	0.0201	0.0132	0.0326*	-0.0239	-0.0100	0.0339	-0.0161	-0.0003
	(0.047)	(0.027)	(0.015)	(0.029)	(0.028)	(0.032)	(0.023)	(0.011)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.0361	-0.0116	0.0145	-0.0500	-0.0168	0.0668*	-0.0213	-0.0015
	(0.061)	(0.034)	(0.021)	(0.026)	(0.033)	(0.030)	(0.017)	(0.015)
Always NMSI Eligible	-0.0176	-0.0213	-0.0024	-0.0010	0.0232	-0.0222	0.0014	-0.0109
	(0.023)	(0.015)	(0.009)	(0.016)	(0.021)	(0.020)	(0.016)	(0.011)
N=	367,996	373,528	373,458	367,265	367,265	367,265	382,092	382,092
Hispanic								
Always Medicaid Eligible	0.0058	-0.0011	-0.0019	0.0021	-0.0482	0.0461	-0.0239	-0.0361
	(0.042)	(0.014)	(0.006)	(0.027)	(0.042)	(0.061)	(0.015)	(0.020)
Medicaid when Pregnant, Ineligible Otherwise	-0.0100	0.0052	0.0024	0.0041	-0.0374	0.0332	-0.0068	0.0017
	(0.038)	(0.013)	(0.005)	(0.026)	(0.031)	(0.046)	(0.013)	(0.012)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.0325	-0.0087	-0.0041	-0.0164	-0.0516	0.0680	-0.0342**	-0.0016
	(0.044)	(0.013)	(0.007)	(0.026)	(0.027)	(0.044)	(0.012)	(0.012)
Always NMSI Eligible	-0.0087	-0.0094	-0.0027	0.0258*	-0.0407	0.0149	0.0118	-0.0001
	(0.021)	(0.009)	(0.003)	(0.012)	(0.030)	(0.027)	(0.006)	(0.014)
N=	761,905	785,636	785,666	765,711	765,711	765,711	792,336	792,336

Notes: Sample consists of women aged 20-45 giving birth for the first time. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

Table 2.5 Detailed Eligibility Interactions on Delivery Outcomes

	Medicaid Paid Birth	Anesthesia	Cesarean Section	LBWT	Preterm	NICU	Breastfeeding
Non-Hispanic White							
Always Medicaid Eligible	-0.0497	-0.0142	0.0348*	0.0088	-0.0054	-0.0043	-0.0778**
	(0.044)	(0.045)	(0.015)	(0.010)	(0.006)	(0.013)	(0.023)
Medicaid when Pregnant, Ineligible Otherwise	0.0114	0.0050	0.0307	0.0062	-0.0094	0.0052	-0.0839**
	(0.041)	(0.048)	(0.017)	(0.010)	(0.007)	(0.013)	(0.027)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.0240	-0.0026	0.0455**	0.0076	-0.0061	0.0096	-0.0484
	(0.042)	(0.043)	(0.014)	(0.010)	(0.007)	(0.013)	(0.027)
Always NMSI Eligible	-.0554*	0.0157	0.0025	0.00901**	0.0057	0.0092	-0.0196
	(0.021)	(0.025)	(0.007)	(0.003)	(0.005)	(0.007)	(0.013)
N=	3,229,760	3,250,799	3,251,405	3,252,863	3,010,514	3,245,957	3,208,427
Non-Hispanic Black							
Always Medicaid Eligible	-0.0074	-0.0117	-0.0120	-0.0169	-0.0182	-0.0366**	-0.0731*
	(0.044)	(0.045)	(0.015)	(0.011)	(0.013)	(0.014)	(0.030)
Medicaid when Pregnant, Ineligible Otherwise	0.0145	-0.0060	-0.0219	-0.0291**	-0.0410**	-0.0239	-0.0758*
	(0.038)	(0.051)	(0.018)	(0.010)	(0.014)	(0.014)	(0.033)
Medicaid when Pregnant, NMSI Eligible Otherwise	-0.0348	-0.0298	0.0108	-0.0066	-0.0104	-0.0041	-0.0825**
	(0.038)	(0.040)	(0.016)	(0.013)	(0.015)	(0.012)	(0.025)
Always NMSI Eligible	-0.0640	0.0611	-0.0513***	-0.0057	-0.0090	-0.0206	0.0253
	(0.035)	(0.040)	(0.013)	(0.010)	(0.010)	(0.014)	(0.032)
N=	695,622	700,270	700,403	700,758	636,512	699,106	688,894
Non-Hispanic Other							
Always Medicaid Eligible	-0.0729	-0.0581	0.0103	-0.0476**	-0.0093	-0.0394	-0.128**
	(0.059)	(0.054)	(0.023)	(0.016)	(0.024)	(0.031)	(0.042)
Medicaid when Pregnant, Ineligible Otherwise	-0.0492	0.0108	-0.0009	-0.0362	-0.0057	-0.0030	-0.0722*
	(0.037)	(0.062)	(0.023)	(0.019)	(0.026)	(0.019)	(0.033)
Medicaid when Pregnant, NMSI Eligible Otherwise	0.0413	0.0055	0.0492	-0.0343*	-0.0310	0.0261	-0.0100
	(0.041)	(0.061)	(0.028)	(0.016)	(0.020)	(0.017)	(0.028)
Always NMSI Eligible	-0.0930**	0.0748	-0.0274	-0.0188	-0.0130	-0.0152	0.0005
	(0.034)	(0.063)	(0.021)	(0.013)	(0.017)	(0.018)	(0.016)
N=	379,322	382,234	382,334	382,478	351,147	381,668	377,344
Hispanic							
Always Medicaid Eligible	-0.149*	0.0009	0.0708*	-0.0241*	0.0069	-0.0221	-0.0690
	(0.060)	(0.068)	(0.030)	(0.010)	(0.017)	(0.022)	(0.037)
Medicaid when Pregnant, Ineligible Otherwise	-0.0812	-0.0068	0.0488	-0.0278*	-0.0050	-0.0056	-0.0465
	(0.060)	(0.053)	(0.025)	(0.010)	(0.018)	(0.021)	(0.034)
Medicaid when Pregnant, NMSI Eligible Otherwise	-0.0833	-0.0869	0.0395	-0.0340**	-0.0164	-0.0148	-0.0307
	(0.068)	(0.048)	(0.027)	(0.012)	(0.020)	(0.016)	(0.034)
Always NMSI Eligible	-0.129***	0.0994	0.0517**	-0.0141	-0.0183	-0.0093	0.0069
	(0.030)	(0.050)	(0.019)	(0.008)	(0.013)	(0.013)	(0.020)
N=	784,791	792,728	792,739	793,008	727,906	792,053	786,073

Notes: Sample consists of women aged 20-45 giving birth for the first time. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. **p* < .05, ** *p* < .01, *** *p* < .001

Again, "Always Medicaid Eligible" and "Medicaid Eligible when Pregnant, Ineligible Otherwise" are comparable to partitions among the Medicaid eligible group while "Medicaid Eligible when Pregnant, NMSI Eligible Otherwise" and "Always NMSI Eligible" correspond to the NMSI eligibility from before. Overall, these results are largely consistent with my previous finding of the results using Medicaid and non-Medicaid eligibility. The signs of coefficients generally agree, particularly in areas where coefficients are significant. However, the more detailed eligibility measures tend to be less precisely measured and noisier. For example, despite the consistent and highly significant effects of NMSI eligibility on smoking related outcomes, only one coefficient among the two detailed eligibility categories that correspond to non-Medicaid eligibility is significant, though most of the signs remain negative.

The main benefit of Table 2.4 and Table 2.5 are the ability to focus on the women gaining subsidized insurance to cover maternity services because they do not qualify for pregnancy-conditional Medicaid. While Table 2.3 found no significant effects of subsidized insurance on the probability that a birth is Medicaid paid, Table 2.5 indicates that a higher share of women "Always NMSI Eligible" leads to a significant reduction in the probability that the birth is Medicaid paid. This finding is significant for all except non-Hispanic blacks. The results suggest that a 10 percentage point increase in share of women eligibility for NMSI but not pregnancy-conditional Medicaid would decrease the probability a birth is Medicaid paid by 2.2% for non-Hispanic whites, 3.1% for non-Hispanic other, and 2.5% for Hispanics. However, the share of women "Always NMSI Eligible" has no clear effects on any of the other pregnancy and birth outcomes.

2.6.2 Robustness checks

In order to test the validity of the assumptions underlying this analysis, I present results from the event study in Table B.1 and Table B.2. The event study contains 360 coefficients that are expected not to be significant (3 DDD style policy variables interacted with 2 years for 4 race-ethnic groups across 15 variables). Among those coefficients, 28 were significant corresponding to 7.8%, which is slightly higher than the 5% expected by chance. Encouragingly the significant coefficients appear dispersed, especially across outcomes. Interactions between variables and the indicator for 2011 conceptions are more commonly significant than those with 2012, and Hispanics have the largest share of significant coefficients across the racial-ethnic groups.

The results from the placebo test are less clean. I split the outcomes up between Tables B.8- B.11. Of the 360 placebo coefficients (2 eligibility measures over 4 race-ethnic groups and 3 time periods across 15 variables), 49 are significant which is 13.6%. Here there is some clustering of significant coefficients over the time lags for some variables among some racial-ethnic groups. This raises some caution regarding the main outcomes for those variables among those racial-ethnic groups. Because there was not a single expansion period, this placebo approach is not a perfect test because some early expanders did have policies prior to January 2014 which could have been had delayed effects on outcomes.

2.7 Discussion

Overall, subsidized insurance eligibility generally has little effect on pregnancy and birth outcomes. The effects are rarely statistically significant, and even when they are, they are generally small. Eligibility for Medicaid prior to pregnancy had no real effect on any of the pregnancy or birth related outcomes, which is somewhat unsurprising given that pregnancy-conditional Medicaid existed for these women prior to the ACA. However NMSI eligibility did have impacts in several important areas: smoking behaviors, the probability that birth was Medicaid paid, and breastfeeding.

One area which theoretically seemed promising to improve is the timeliness of prenatal care. Prior to the ACA, women using Medicaid to pay for their pregnancies may have delayed prenatal care because of lack of awareness of their eligibility, delays in enrolling, or the emotional burden of navigating an unfamiliar insurance system. Being eligible for subsidized insurance prior to conception then could potentially reduce some of those delays. The results, however, do not indicate this. Across all racial and ethnic groups, I determine that a 10 percentage point increase in Medicaid or non-Medicaid subsidized eligibility changed the probability a woman had a prenatal care visits in the first trimester by less than 0.75 percentage points in either direction, according to a 95% confidence interval. Thus this null effect is fairly precisely estimated and noteworthy in its own right.

NMSI eligibility, stemming mainly from Marketplace plans, appears to reduce smoking among mothers. The results indicate a 10 percentage point change in non-Medicaid subsidized eligibility reduced the probably of smoking prior to pregnancy by 2.6% for non-Hispanic whites, 5.9% for non-Hispanic other, and 2.7% for Hispanic women. While subsidized insurance may increase access to medical providers who can help people quit, the ACA also contained mandates to encourage quitting. Private plans, including Marketplace plans, can charge smokers up to 50% higher premiums according to the ACA, though some states set this limit lower (Kaplan et al., 2014). Additionally, tobacco cessation counseling and interventions are preventative services which must be provided without cost sharing or prior authorization, and participants using services can have the premium penalty waived (McAfee et al., 2015). However, if women are now more concerned that their insurers may penalize them for cigaret1te use, it is also possible that underreporting of cigarette use increased.

Given the sample for both pre-pregnancy and third trimester cigarette use is all mothers, the reduction in pre-pregnancy smoking should also be reflected in a reduction of third trimester smoking; however, the effects of the NMSI eligibility on cigarette use in the third trimester are less significant, though the coefficients still indicate a reduction. This could suggest that the women who reduce cigarette use prior to pregnancy as a result of this insurance expansion are the ones who would have quit smoking during their pregnancy without the expansion. This is consistent with the idea that women who smoke in the third trimester are the ones with the greatest difficulty in quitting. Alternatively, the results may lack significance because the incidence of third trimester cigarette use is fairly rare, making detecting a significant change difficult.

When partitioning the eligibility groups further in order to highlight the interactions between these subsidized insurance programs for non-pregnant childless women and pregnancy-conditional Medicaid, I find women who are always eligible for Marketplace plans even when pregnant (Group 4) were less likely to have a Medicaid paid birth. The result holds across all racial-ethnic groups, and particularly significant among the non-Hispanic other and the Hispanic groups. This is consistent with women prior to the Marketplace expansion lowering their incomes in order to qualify for Medicaid coverage for their pregnancy. Current studies on the ACA and employment outcomes generally find no sizable effects on employment effects (Bailey, 2017; Heim et al., 2017; Kaestner et al., 2017). These results suggest that labor responses of pregnant women may be more responsive and further research should be done on the employment effects for this special group.

The lack of significant changes in nutritional outcomes such as adequate weight gain, gestational diabetes, or gestational hypertension are somewhat expected given that these behavioral outcomes can be hard to modify and would likely stem from better prenatal care, which did not appear to improve. More interesting is that the share eligible for NMSI led to no significant changes in birth procedures such as anesthesia or cesarean sections. This is even true for the share "Always NMSI Eligible" which reflects women who are ineligible for pregnancy-conditional Medicaid but qualify for other subsidized insurance plans which cover maternity services. It is likely that women using Marketplace plans to pay for births would not have had self-paid births otherwise since the share of self-paid births had been consistently below 5%. It is more likely that these women use Marketplace plans for birth rather than lowering their incomes to qualify for Medicaid, as evident by decreases in Medicaid-paid births. In this case, women might actually end up paying more for births due to copays for maternity services as opposed to free maternity services under Medicaid.

Finally, there were no clear changes in the incidence of LBW or preterm babies, which is unsurprising given the lack of significant changes in prenatal care and other pregnancy related outcomes. Additionally, policies and interventions which significantly improve birth outcomes have been evasive.

2.8 Conclusion

The ACA overhauled the U.S. health care system with the intention of improving insurance coverage through a variety of mandates and an increase in subsidized insurance programs. Prior to the ACA, young adults were the segment of the population with the highest rates of uninsurance (C. Courtemanche et al., 2016), and the highest fertility rates. While pregnancy-conditional Medicaid has been around since the mid-1980s to cover maternity services for low-income women, higher income women without access to ESI faced difficulties finding non-group insurance plans which provided maternity services at an affordable price. Furthermore, delays in prenatal care for Medicaid paid births raised concerns about lack pregnancy-conditional Medicaid awareness or delays in enrolling. The ACA created subsidized insurance programs for non-pregnant women—

Medicaid for those up to 138%FPL in expansion states and subsidized Marketplace plans for women 100 to 400%FPl without access to another affordable, minimum essential coverage plan. Included in these plans was coverage of maternity services, no more restrictions on preexisting pregnancy conditions, and free preventative care which included prenatal care and breastfeeding consultations.

These significant increases in pre-pregnancy insurance coverage, and additional increases in maternity service coverage for women with incomes above their states' pregnancy-conditional Medicaid income thresholds, produced few changes in pregnancy and birth related outcomes. I can rule out changes larger than 0.75% in the probability a mother had a prenatal care visit during the first trimester given a 10 percentage point increase in subsidized insurance eligibility. These eligibility expansions also do not seem to affect pregnancy nutrition outcomes such as maternal weight gain, gestational diabetes, or gestational hypertension. In addition, there are no consistent changes in the intensity of delivery procedures such as anesthesia or cesarean section or in birth outcomes such as the incidences of LBW and preterm infants.

Areas where there are significant changes align with specific ACA mandates. Results indicate a higher share of women eligible for NMSI leads to significant reductions in pre-pregnancy cigarette use across all racial-ethnic groups other than non-Hispanic blacks. This eligibility also increases the probability the mother was breastfeeding when leaving the hospital, particularly for non-Hispanic other and Hispanic groups. However, when using more detailed eligibility measures which account for pregnancy-conditional Medicaid eligibility, the measures of eligibility for women with non-Medicaid eligibility prior to their pregnancy do not produce significant effects for these outcomes, though the sign of the coefficients match those of the significant non-Medicaid eligibility coefficients.

On the other hand, the share eligible for non-Medicaid insurance prior to pregnancy does not significantly reduce the probability a birth is Medicaid paid, but honing in on those women who are also ineligible for pregnancy-conditional Medicaid indicates a significant reduction in the probability a birth is Medicaid paid. This suggests that following the ACA, women without access to ESI plans may use Marketplace plans to pay for maternity services whereas they would have made themselves eligible for pregnancy-conditional Medicaid otherwise.

This analysis faces limitations. First, the majority of the outcomes rely on self-reported information, which may be inaccurate. This is particularly problematic when such misreporting may be correlated with the expansion of ACA expansion, which may be the case for cigarette reporting since the ACA allows private insurance companies to charge smokers higher premiums. Additionally, these results do not account for changes in the population of women who are giving birth due to the ACA. If the composition of mothers contains a higher share of intended pregnancies due to better contraception for women not intending to become pregnant and better maternity coverage for those desiring to be, utilization and health outcomes may improve due to the sample selection into motherhood.

The ACA targeted improving insurance coverage, particularly for low-income, young adults and those with high medical expenses. While low-income, would-be mothers largely match these categories, pregnancy-conditional Medicaid which existed prior to the ACA was covering pregnancy related expenses for many of these women. Thus while the ACA did significantly improve access to subsidized insurance eligibility, the effects on pregnancy and birth related health care utilization and outcomes were minimal. Consistent with mandates within the ACA, my results suggest higher shares of women eligible for NMSI decreased smoking, Medicaid-paid pregnancies, and slightly increased breastfeeding.

CHAPTER 3. MEDICAID MANAGED CARE AND THE HEALTH CARE UTILIZATION OF FOSTER CHILDREN

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3.1 Introduction

As of September 2014, over 415,000 children in the United States were enrolled in the foster care program (US Department of Health Human Services, 2015). It is well established that foster children are a medically vulnerable population due to their histories of abuse and neglect (Child Welfare Information Gateway, 2015). One recent study found that foster children were more likely to have developmental disorders, certain medical disorders, and behavioral disorders than non-foster Medicaid children (Center for Mental Health Services and Center for Substance Abuse Treatment, 2013). Practically all foster children are categorically eligible for Medicaid, (Child Welfare Information Gateway, 2015) and while as a group they make up only 3.7 percent of the non-disabled children enrolled in Medicaid, they are responsible for 12.3 percent of expenditures for this group due to their high levels of health needs (Geen, 2005).

There is often political tension between the benefits of safety net programs like Medicaid and their associated costs (Marton & Wildasin, 2007). This tension, along with a desire to improve care coordination as well as health outcomes, has led many states to transition their Medicaid populations from traditional fee-for-service (FFS) coverage to Medicaid managed care (MMC) coverage (Marton, Talbert, et al., 2016). When managed care organizations (MCOs) contract with state Medicaid agencies, they agree to receive a fixed (capitated) payment based on the number of enrollees and their characteristics. Because this payment does not depend on the amount of services provided, MCOs bear the financial risk associated with the care for these enrollees. They are thus incentivized to reduce overall health care utilization and spending through improvements in the health status of their enrollees (Day et al., 2016; Marton et al., 2014).

In the late 1990s there was a large movement within Medicaid towards managed care and by mid-1998 more than half of Medicaid enrollees were enrolled in a managed care plan (Kaye et al., 1999). Initially states needed to obtain waivers from Centers for Medicare and Medicaid (CMS) to require beneficiaries to enroll in a MMC plan, but the Balanced Budget Act of 1997 allowed states to make MMC mandatory for most eligibility categories. However, since 1997 foster care has continued to be one eligibility category which requires waivers for mandatory MMC (Leslie et al., 2003; National Association of Social Workers, 1997). Consequently, in 1998, there were 45 states that had at least one MMC plan, of which, 16 excluded foster children and 9 allowed them to disenroll from what otherwise would have been a mandatory plan (Kaye et al., 1999). More recently several states have sought approval from CMS to transition their aged and disabled populations into MMC as well (Van Parys, 2014).

One reason waivers are required to implement mandatory MMC for vulnerable populations, such as foster children, is the concern that MCOs may reduce spending by limiting access to needed medical care rather than reducing wasteful care (Marton et al., 2014). One might expect this to be particularly problematic for foster children since their high levels of health care utilization may be misinterpreted as excessive spending by managed care plans rather than reflecting greater health needs. Additionally, foster children perhaps lack parents that can be considered reliable health care advocates on their behalf, so they may be more likely to be targeted for across the board reductions in care by managed care plans. On the other hand, a transition to managed care, with its focus on care coordination, might be beneficial for foster children since they might be especially prone to having uncoordinated health care due to the circumstances precipitating their entry into the foster system (Allen, 2008; Marton, Yelowitz, et al., 2016). Thus the impact of MMC on the health care utilization of foster children is theoretically ambiguous and requires empirical analysis.

Related to concerns surrounding the potential for changes in utilization are concerns surrounding changes in continuity of care. Continuity of health care for foster children can be particularly challenging since placement changes may also cause a change in doctors (Kerker & Dore, 2006). A study of Medicaid children in Washington found foster children had less continuous care than non-foster children, and furthermore found that among non-foster children, those in MMC had more continuous care than those in FFS Medicaid (DiGiuseppe & Christakis, 2003). If this pattern holds true broadly, MMC may improve continuity of care for foster children.

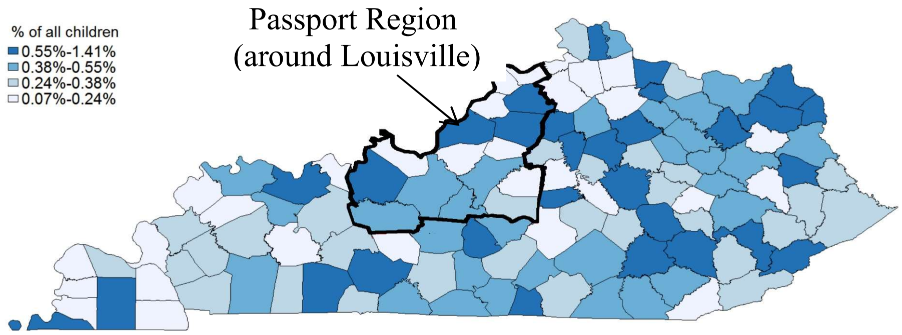
It is useful for states to know how managed care affects the health care utilization of foster children. If managed care does not reduce health care utilization among foster children, then continuing to have an eligibility carve out for this group makes little sense. On the other hand, if care is reduced, it is important for states to know by how much and if the reduction is stemming from less waste or restricted access to necessary care. However, because it is often challenging for researchers to obtain large datasets with information on both health care utilization and foster status of children, the impact of MMC on the health care utilization of foster children is still an open question in the literature. A non-capitated managed care program for foster children in Illinois that aimed to increase care coordination was associated with increased well-child visits, though it is possible the foster children had more visits because they had higher needs than children in the comparison groups (Jaudes et al., 2004; Jaudes et al., 2012). A nationally representative study on children in the child welfare system, though not exclusively in foster care, found no significant relationship between managed care and access to outpatient mental health services (Raghavan et al., 2006). One challenge associated with these studies is the lack of an adequate control group.

This paper takes advantage of the unique way in which foster children in Kentucky Medicaid were moved into managed care coverage in 1999 in order to evaluate the short run impact of managed care on their outpatient health care utilization. Foster children in the Louisville region of Kentucky were mandatorily moved into MMC in June 1999 while foster children in the remainder of the state remained in FFS (Bartosch & Haber, 2004). We compare the health care utilization of foster children in the Louisville region in the first and second half of the year with the health care utilization of foster children in the rest of the state. This difference-in-differences (DD) research design allows us to isolate the causal effect of MMC on the health care utilization of foster children.

3.2 Data and Methods

3.2.1 Natural experiment

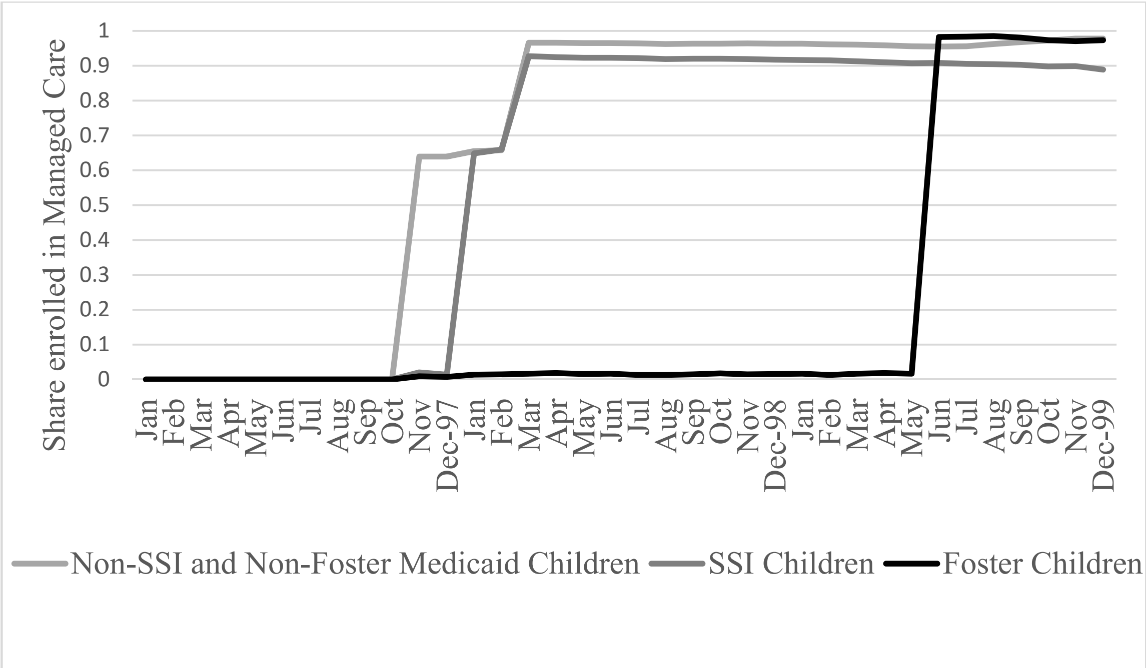
In October 1995, CMS approved a waiver for Kentucky to move its Medicaid population into managed care plans. Originally managed care markets were to be developed in eight regions partitioning the state, but ultimately only one managed care plan (Passport) operating in one region (Louisville) was able to both successfully establish operations and remain financially viable. The fact that there were significant numbers of foster children both inside and outside the Louisville area suggests the possibility of a comparative analysis. Figure 3.1 Map of Kentucky Region and Country Share of Medicaid Foster Children in January 1999. Figure 3.1 illustrates how Medicaid foster children were distributed throughout the Louisville region and all other parts of the state as of January 1999.



Notes: We calculated the foster shares presented here using two data sources: i) Medicaid foster enrollment data from January 1999 provided by the Kentucky Cabinet for Health and Family Services and ii) County child count data come from U.S. Census April 2000 resident population estimates.

Figure 3.1 Map of Kentucky Region and Country Share of Medicaid Foster Children in January 1999

Medicaid children within the Louisville region were mandatorily enrolled in Passport, but there was a delayed roll out by eligibility category, which is shown in Figure 3.2. The majority of children, such as those eligible for Medicaid via enrollment in the Temporary Assistance for Needy Families (TANF) program, transitioned starting in November 1997. Children jointly enrolled in Medicaid and the Supplemental Security Income (SSI) program (i.e. children who are blind or disabled) transitioned a few months later. Foster children, however, did not transition into Passport until June 1999, a year and a half after the plan started. Not only was the timing of the transition different among eligibility groups, but so were the capitation rates that the state paid Passport. For the 1999 fiscal year, Kentucky paid Passport \$146.20 per TANF child-month, \$531.51 per SSI child-month, and \$188.52 per foster child-month.[20] The state required Passport to report encounter data in a similar fashion to the claims reported pre-MMC. Because Passport was formed by local providers, they did not appear to find this to be as burdensome a requirement as would a commercial MCO coming in from outside the state.



Source: De-identified, linked Medicaid claims and enrollment data provided by the Kentucky Cabinet for Health and Family Services.
Notes: The pre-reform time period is January 1999 to May 1999 while the post-reform time period is June 1999 to December 1999.

Figure 3.2 Timing of Medicaid Managed Care Transitions for Children in the Louisville Region Kentucky

Since Kentucky chose when, where, and which eligibility categories to move into MMC, there is no endogenous selection into insurance types. In other words, foster families could not choose whether their foster child would be enrolled in managed care or FFS coverage. This implies the state essentially conducted an experiment where it assigned foster children in the Louisville area into the MMC treatment (i.e. Passport) and all other foster children into the FFS control. As is shown in Figure 3.2, the transition to Passport was particularly sharp for foster children as compared to the transition for other eligibility categories. The percent of foster children enrolled in Passport in the Louisville region went from 1.7 percent in May 1999 to 98.2 percent in the following month.

3.2.2 Data

Using linked administrative data from the Kentucky Cabinet for Health and Family Services for the calendar year 1999, we evaluate the differential impact of the managed care transition of foster children. Our work with this data is covered under University of Kentucky Institutional Review Board Protocol number 05-0795-X4G. To construct our sample we started with 9,469 unique children who were enrolled in the Kentucky Medicaid program and were in foster care for at least one month during 1999. We then restricted the sample to those continuously enrolled in foster care for all 12 months of 1999, leaving us with 4,325 unique children. After dropping children with missing values for key variables of interest, our final sample consists of 4,315 unique children continuously enrolled in Kentucky foster care and Medicaid for all of 1999. Having the universe of Kentucky Medicaid administrative data for this time period allows us to focus on the very specific sub-set of enrollees of interest for this analysis (i.e. continuously enrolled foster children), while still having sufficient sample size to estimate the effect of MMC.

Our outcome variables focus on outpatient services along both the extensive and intensive margins. Following previous analysis of Kentucky Medicaid (Marton et al., 2014), we define outpatient services to be services delivered in clinics or hospitals in which there is no overnight stay (such as an emergency room visit). These visits do not include primary care provider visits. Along the extensive margin, we consider the probability that a child will have an outpatient visit within a given month. Along the intensive margin, we examine monthly outpatient spending, conditional on having any outpatient utilization within that month.

3.2.3 Data Analysis

We employ a DD regression framework to determine the causal effect of the Passport MMC plan on health utilization for foster children. This method compares how outpatient utilization changed for foster children in the Louisville region after their switch to the Passport MMC plan relative to foster children throughout the rest of the state who remained in FFS Medicaid. By having a control group of foster children who are exposed to the same state trends, but not the MMC transition, we obtain an unbiased estimate of how much of the change in outpatient utilization resulted from the MMC transition. The identifying assumption is that outpatient utilization trends for these two groups are initially similar and would have continued to be similar in absence of the MMC transition.

Since foster children transitioned to MMC based on whether their county of residence was in the Passport region, we based our treatment variable on the foster child's county of residence in January 1999, prior to the policy implementation. This is sometimes referred to as an "intent-to-treat" approach. There was almost no migration in or out of the Louisville region during 1999 among our sample (only 0.83 percent switched regions), so our choice to use initial month to assign treatment status is inconsequential.

Our regressions include child fixed effects to measure the intra-child variation in outpatient utilization. The inclusion of child fixed effects controls for time-invariant child characteristics, like race or gender, whether they are observed or not. For this reason the standard time invariant controls employed in the literature are excluded here because of multicollinearity. Perhaps more importantly, the inclusion of child fixed effects also allows us to control for child chronic health conditions, which might influence health care utilization. We also include time fixed effects in the form of month dummies to capture seasonal variation in health care utilization. Finally, we compute heteroscedasticity-robust standard errors clustered at the county level in all of our regression models.

We separately measure how the MMC transition affected outpatient care along the intensive and extensive margin. The extensive margin regressions measure the probability of a child having an outpatient visit during that month and are estimated as linear probability models. For the intensive margin regressions, the outcome is the log of outpatient expenditures conditional on some positive outpatient utilization in that month and the regressions are estimated using ordinary least squares.

3.3 Results

In this section, we first report our unadjusted descriptive results given in Table 3.1. We then turn to a presentation of our multi-variate DD regression results given in Table 3.2 and Table 3.3.

3.3.1 Descriptive Results

Table 3.1 reports descriptive statistics for our sample, splitting the sample by their MMC status based on their initial county of residence. Recall that foster children living in the Louisville region of Kentucky were transitioned from FFS to MMC coverage in June of 1999 (i.e. the treatment group) while foster children living in the rest of the state remained in FFS Medicaid coverage (i.e. the control group). In terms of demographics, we see in the top panel that the biggest difference is that the treatment group has a larger share of non-white enrollees (47 percent vs. 23 percent).

The middle panel compares outpatient utilization along the extensive margin in the pre-transition (January-May 1999) time period and the post-transition (June-December 1999) time period. We see that in the pre-period the likelihood of a foster child having any monthly outpatient utilization is 6 percent in the treatment group, as compared to 9 percent for the control group. The likelihood of having any monthly outpatient utilization within the treatment group after they are transitioned to MMC decreases significantly from 6 percent to 3 percent (p-value < 0.01), while it stays about the same for the control group (8.59 versus. 8.54 percent, p-value = 0.85).

Table 3.1 Descriptive Statistics

	Foster Children Moved to MC Medicaid (treatment)	**Foster Children Remaining in FFS Medicaid (control)**	**Difference (control group-treatment group)**
# children	1,448	2,867	1,419
# child-months	17,376	34,404	17,028
Demographics:			
% non-white	47.03%	22.60%	-24.43%***
% female	48.55%	50.26%	1.71%***
Avg. age on Jan 1, 1999	9.87	9.70	-0.17***
Avg. # siblings	0.11	0.097	-0.013***
Utilization (percentage with any monthly Medicaid utilization):			
Outpatient - pre	6.38%	8.59%	2.21%**
Outpatient - post	2.67%	8.54%	5.87%***
Expenditures | Expenditures > 0 (amount of monthly Medicaid spending):			
Outpatient ($) - pre	$274.54	$282.04	$7.50
Outpatient ($) - post	$102.01	$273.32	$171.31***

Source: De-identified, linked Medicaid claims and enrollment data provided by the Kentucky Cabinet for Health and Family Services.
Notes: The pre-reform time period is January 1999 to May 1999 while the post-reform time period is June 1999 to December 1999. The stars represent the results of tests for difference in means or proportions between the treatment and control groups.
*Statistically significant difference at 5% level, **Statistically significant difference at 1% level, ***Statistically significant difference at 0.1% level.

The bottom panel compares monthly Medicaid outpatient expenditures, conditional on having positive monthly outpatient Medicaid spending. Here we see very similar levels of outpatient average spending in the pre-period for foster children in the treatment group and the control group. In the post period, there is a large reduction in average outpatient spending among foster children in the treatment group (p-value < 0.01). There is no statistically or meaningfully significant change in average outpatient spending within the control group (p-value = 0.68).

Therefore, for outpatient services, Table 3.1 provides suggestive evidence of larger reductions in utilization along both the intensive and extensive margin for foster children transitioned to MMC, as compare to the control group of foster children remaining in traditional FFS Medicaid.

3.3.2 Regression Results

Table 3.2 presents the results of our baseline DD multivariate regression analysis. We find that MMC enrollment is predicted to lead to a 4 percentage point (51 percent) decline in the probability of receiving any monthly outpatient services. Thus managed care leads to a reduction in outpatient service utilization along the extensive margin (i.e. did a child have any visit?) for foster children. We also examined changes along the intensive margin (i.e. how much?) for months with non-zero levels of outpatient spending. Our results suggest that managed care also led to reductions in monthly outpatient spending along the intensive margin. Therefore we see evidence that managed care led to reductions in the probability of foster children having an outpatient visit and in outpatient expenditures conditional on using such care. This finding of reductions along both margins is similar to findings for non-foster children transitioning into MMC in Kentucky (Marton et al., 2014).

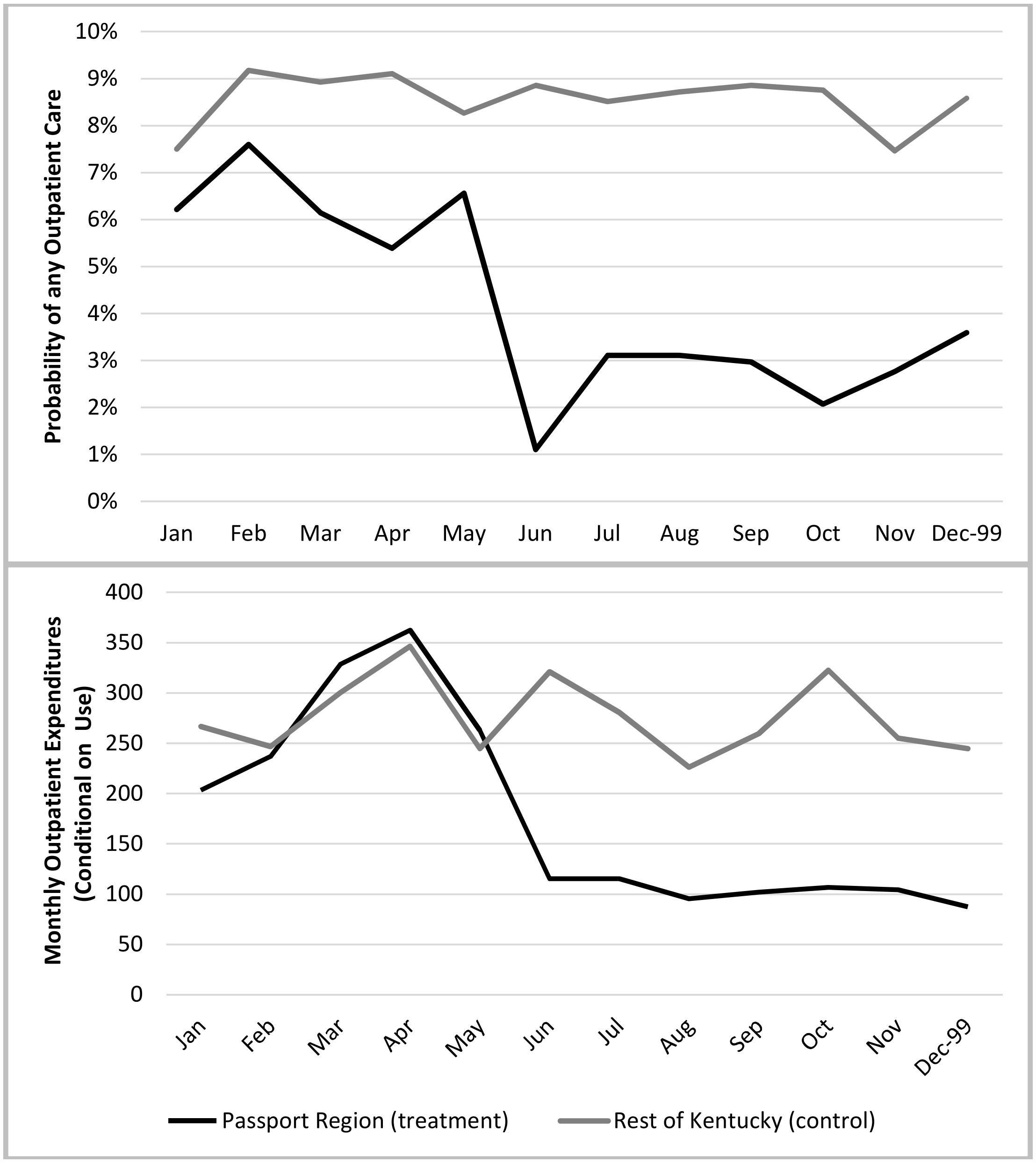
As mentioned, the identifying assumption underlying our DD analysis is that the outpatient utilization trends for foster children inside (treatment) and outside (control) of the Louisville region are similar prior to the MMC transition. Figure 3.3 separately plots trends for outpatient utilization for both the extensive and intensive margin. The graph on the top focuses on the extensive margin (the probability of any monthly outpatient utilization) and graph on the bottom focuses on the intensive margin (outpatient expenditures conditional on having positive expenditures). Both graphs exhibit relatively similar trends in the pre-reform period for the treatment and control groups. To be more specific, regressing the outpatient care variables on pre-period linear time trends separately for treatment versus control regions indicates that the pre-period trends between the regions are not statistically different at the 5 percent level. This implies we can interpret our results causally. Despite similar pre-treatment trends, reductions in outpatient utilization occur for the treatment group in the post period but are not observed for the control group. In fact, the outpatient utilization of the control group of foster children living outside the Louisville region remains essentially constant throughout the year. This is consistent with the reduction in outpatient utilization among foster children we observe being caused by their transition to MMC.

Table 3.2 Regression Results

Dependent Variable	**Probability of having an outpatient visit**	**Log expenditure conditional on outpatient visit**
MMC Enrollment (Standard Error)	-0.04*** (0.004)	-1.26*** (0.178)
% Change	-50.96	-71.64%
Pre-reform average monthly utilization / spending	7.85%	$279.99
Observations	51,780	3,678

Source: De-identified, linked Medicaid claims and enrollment data provided by the Kentucky Cabinet for Health and Family Services.
Notes: The pre-reform time period is January 1999 to May 1999 while the post-reform time period is June 1999 to December 1999. The stars represent the results of tests for difference in means or proportions between the treatment and control groups. Regressions include month fixed effects and child fixed effects.
*Statistically significant difference at 5% level, **Statistically significant difference at 1% level, ***Statistically significant difference at 0.1% level

In order to add further support for our finding that the reduction in outpatient care among foster children in the Louisville region is driven by the implementation of MMC, we perform multiple robustness checks. First, we conduct a placebo test where we used Lexington, an urban region similar to Louisville but which did not transition foster children into MMC, as a placebo treatment group. Treating Lexington as if it implemented MMC for foster children in June 1999, we estimate a DD model comparing it to the rest of the state, dropping the Louisville region entirely. Reassuringly, as reported in Table 3.3, we find no statistically significant differences in outpatient utilization between the placebo treatment group and the control group. This suggests



Source: De-identified, linked Medicaid claims and enrollment data provided by the Kentucky Cabinet for Health and Family Services.
Notes: The pre-reform time period is January 1999 to May 1999 while the post-reform time period is June 1999 to December 1999.
Figure 3.3 Kentucky Foster Care Utilization Trends in 1999

that there was not some general trend reducing outpatient utilization in all urban areas among foster children during this time. Second, we replicate our baseline DD specification using a broader sample of foster children in which continuous enrollment during calendar year 1999 was not required. The results, also reported in Table 3.3, are similar to our baseline specification with our continuously enrolled sample. This suggests our results are not being driven by selection into continuous enrollment. Finally, we replicated our baseline DD specification using only foster children in the urban Lexington region (rather than all regions besides Louisville) as the control group. The results, which are available upon request, were again similar to those reported in Table 3.2.

Table 3.3 Robustness Tests

Dependent Variable	**Probability of having an outpatient visit (placebo treatment group)**	**Log expenditure conditional on outpatient visit (placebo treatment group)**	**Probability of having an outpatient visit (non-continuous foster enrollment)**	**Log expenditure conditional on outpatient visit (non-continuous foster enrollment)**
MMC Enrollment (Standard Error)	-0.01 (0.005)	0.08 (0.116)	-0.03*** (0.004)	-1.30*** (0.122)
% Change	-12.42%	8.33%	-38.22%	-72.75%
Pre-reform average monthly utilization / spending	8.05%	$282.04	7.85%	$279.99
Observations	34,404	2,945	77,874	6,032

Source: De-identified, linked Medicaid claims and enrollment data provided by the Kentucky Cabinet for Health and Family Services.
Notes: The pre-reform time period is January 1999 to May 1999 while the post-reform time period is June 1999 to December 1999. The stars represent the results of tests for difference in means or proportions between the treatment and control groups. Regressions include month fixed effects and child fixed effects.
*Statistically significant difference at 5% level, ** Statistically significant difference at 1% level, ***Statistically significant difference at 0.1% level

3.4 Discussion

This study is one of the first to empirically investigate how the transition from FFS Medicaid to MMC affects the health care utilization of foster children. While many studies have examined the effects of MMC in general, quantitative research focusing on foster children and MMC is almost nonexistent due to inherently smaller sample sizes and fewer MMC mandates for such children. As of 2013, only 17 states had a comprehensive Medicaid MCO which mandatorily enrolled foster children (Centers for Medicare and Medicaid Services, 2015). This is likely due in part to that fact that MMC mandates for foster children require approval from CMS, a policy that likely stems from concerns that MMC may reduce access to necessary care for this vulnerable population. However, there is little evidence to indicate how serious those concerns are. The "natural experiment" that occurred with respect to MMC and foster children in Kentucky, which we exploit in this paper, is useful for obtaining causal estimates of the effect of MMC on foster children's health care utilization.

Because foster children have higher levels of chronic health conditions, it is important that MMC plans are paid higher capitation rates for this eligibility category in order to cover their necessarily higher costs (Harman et al., 2000; Leslie et al., 2010). If the capitation rates are not higher, plans would have increased pressure to reduce health care utilization for foster children in order to remain profitable. As mentioned, Kentucky provided a 28.9 percent higher capitation rate for foster children than for TANF children. Perhaps due in part to this difference in financing, the reductions in outpatient utilization we find for foster children (51 percent) is similar to or lower than the estimate produced when examining all Medicaid children (61 percent) (Marton et al., 2014). Taken together, our results suggest that while MMC did reduce outpatient utilization among foster children, these reductions were smaller than those experienced by other Medicaid children.

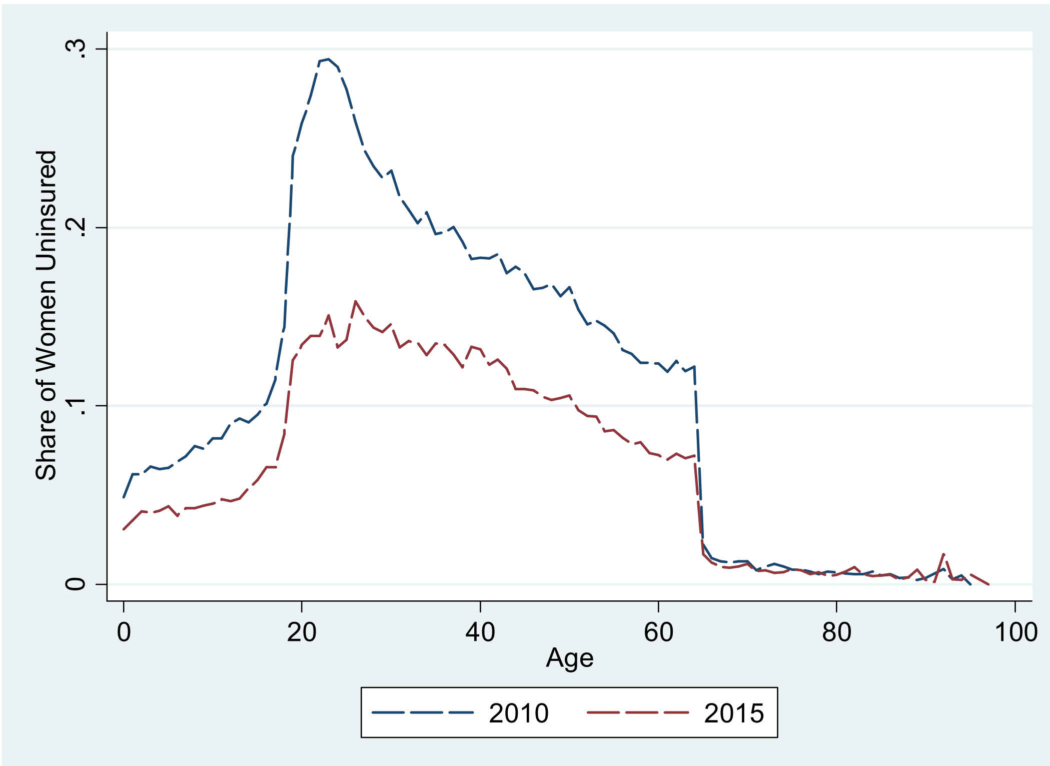
This is consistent with, thought may not necessarily imply, that Passport maintaining reasonable access to care for foster children while producing resource savings.

Previous work examining the association between the transition from FFS to MMC and time to first-visit for new entrants into foster care and find that the transition is associated with an improvement in the timeliness of initial well-child visits (Day et al., 2016). The results from our paper provide a fuller picture, using a methodology that accounts for confounding trends with a DD framework. Our work examines children who are already in the foster care system for a non-trivial amount of time (January to June, 1999), and finds reduced frequency of outpatient visits after MMC. Taken together, one could interpret the findings as suggesting that managed care better coordinates care, resulting in timely initial visits for children, and such visits reduce the need for subsequent outpatient utilization. Additional research is needed to rule out competing interpretations.

We qualify our findings in light of some limitations of our study. The primary limitation is that we are not able to differentiate between reductions in wasteful and necessary outpatient care. If MMC solely reduced unnecessary care, the findings would be unambiguously positive. Of course, differentiating between wasteful and necessary care can be a major challenge without objective measures of health needs. This warrants additional studies that are able to extend both our work and previous work (Day et al., 2016; Landers et al., 2013) to consider the impact of MMC on health outcomes. Secondly, in order to have complete information on outpatient utilization, we used foster children who were continuously enrolled for 12 months. These foster children are not fully representative of foster children in general because there is a good deal of turnover within this population. Our approach does not seem to be an overly restrictive, as 68 percent of foster children in Kentucky in January 1999 had continuously been in foster care for at least a year. Additionally, our study measures short run utilization effects that occur within the first 7 months after the transition. Longer term studies would help determine if the short run reductions in utilization we observe persist. Furthermore, as this paper focuses on outpatient care, we are unable to shed light on shifts between different types of health care utilization. We cannot determine if the reduction in outpatient care we observe arose because all health care utilization fell or because foster children substituted other types of care for outpatient care. Further research is needed to investigate this sort of substitution.

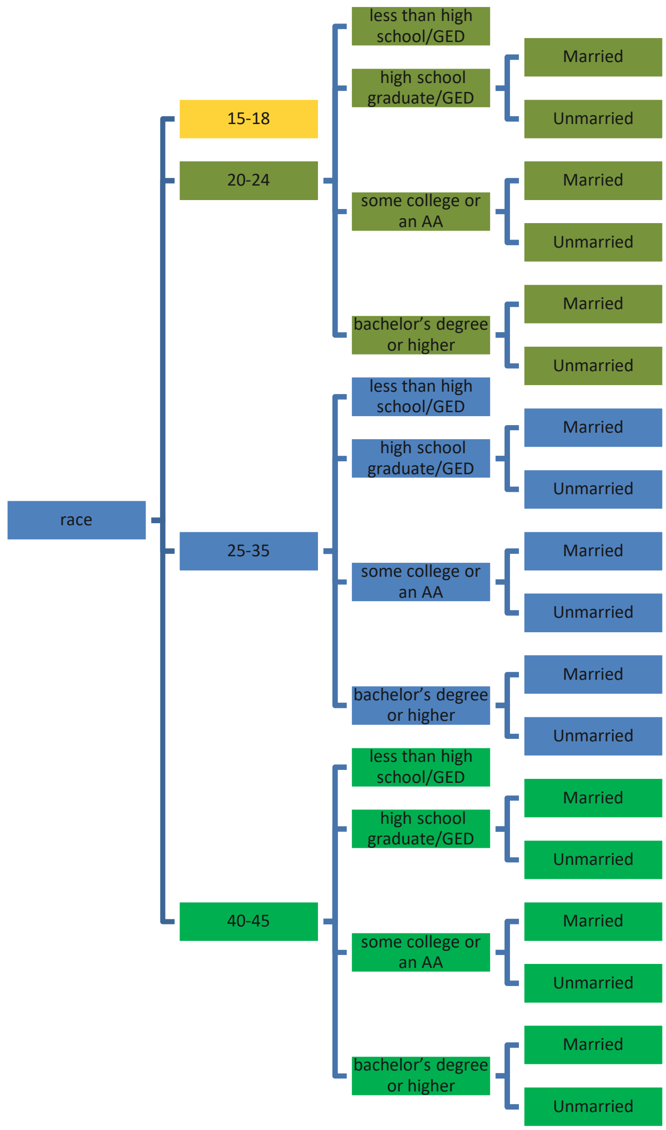
Finally, while the age of our data may limit the external validity of the results, the unique natural experiment in Kentucky we exploit in which we are able to not only measure foster child utilization before and after the policy change, but to do so with reference to a control group of foster children, provides the benefit of a high level of internal validity. The causal evidence we find therefore provides an important contribution to the literature despite the cost of using older data. This is especially true given that there is practically no previous empirical research examining the impact of MMC on the health care utilization of foster children. As more states transition their foster care populations into mandatory MMC, researchers should monitor how this vulnerable population is affected in order to better assess the costs and benefits of MMC.

APPENDIX A. SUPPLEMENTAL MATERIAL FOR CHAPTER 1



Source: ACS data

Figure A.1 Uninsurance Rate for Females by Age



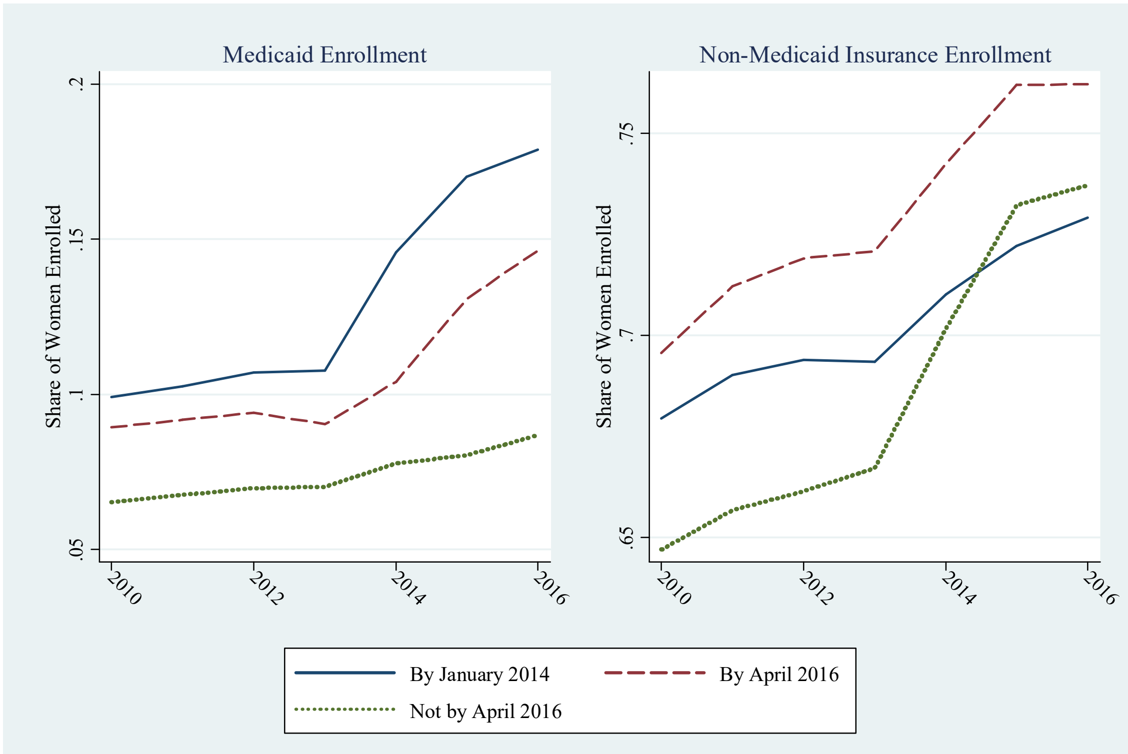
Notes: This flow chart shows the 22 groups created for each of 4 race/ethnicity groups: non-Hispanic white, non-Hispanic black, non-Hispanic other race, and Hispanic. This gives a total of 88 demographic groups.

Figure A.2 Flow Chart of Demographic Groups

Table A.1 Pre-ACA State Plans

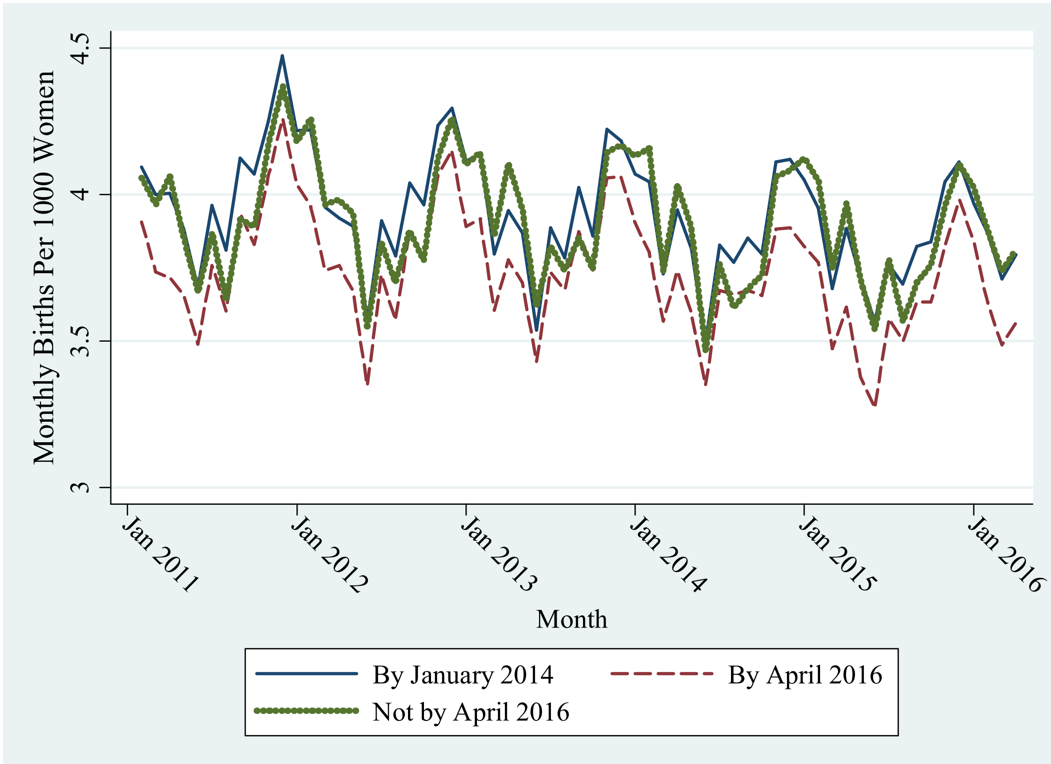
State	2009	2010	2011	2012	2013
Arizona*	100	100	100	0	0
California*	0	0	200	200	200
Colorado*	0	0	0	10	0
Connecticut*	67	67	67	67	67
Delaware*	108	108	108	108	108
DC*	210	210	210	210	210
Hawaii*	107	107	107	107	107
Iowa	240	240	240	240	240
Maine	300	300	300	300	300
Maryland	123	123	123	123	123
Massachusetts	300	300	300	300	300
Minnesota*	253	253	253	253	253
New Jersey	0	0	0	25	25
New Mexico	205	205	205	205	205
New York*	110	110	110	110	110
Oklahoma	223	223	223	223	223
Oregon	220	220	220	220	0
Utah	105	105	105	105	105
Vermont*	307	307	307	307	307

*Note*s: These table shows income thresholds for plans that were not waitlisted and were not dependent on an enrollee's employer. States that had plans that reached capacity or were discontinued between 2009 and 2013 are shown as having an income threshold of 0, implying that the plan is not taking any more enrollees. The asterisk denote plans which KFF classifies as Medicaid (KFF, 2017a).



Source: ACS

Figure A.3 Insurance Enrollment by When States Expanded Medicaid



Source: US natality records, the ACS, and the Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data.

Figure A.4 Monthly Birth Rates by When States Expanded Medicaid

A.1 Policies and Eligibility Groups Example

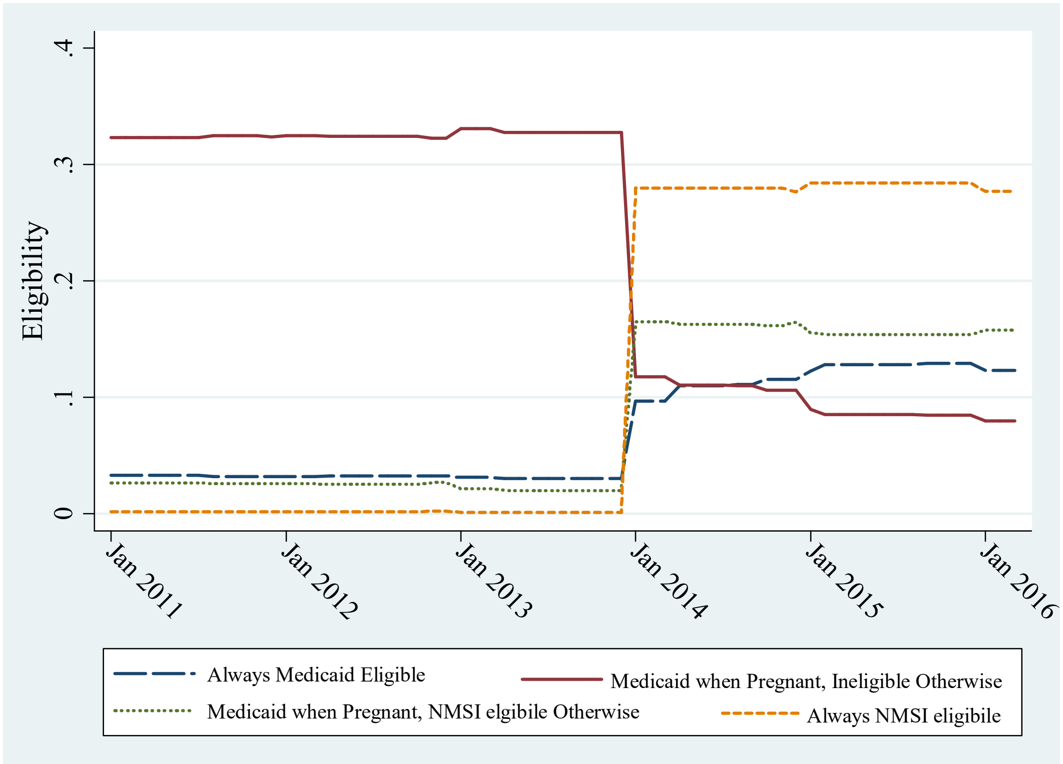
This section shows policies for pregnancy-conditional Medicaid, Medicaid, and Marketplace plans in Nevada as an example state. Nevada had no childless adult plans prior to the Affordable Care Act and expanded Medicaid effective January 1, 2014. The pregnancy-conditional Medicaid income threshold remained at 165% over the period. Table A.2 shows who was eligible for different programs before and after the ACA expansion. In the baseline specifications, I focus on childless, non-pregnant adults to classify eligibility into women eligible for Medicaid and women eligible for NMSI. Table A.3 maps the policies for pregnant and non-pregnant, childless adults into the detailed eligibility groups which reflect the interactions between subsidized insurance programs.

Table A.2 Subsidized Insurance Program Policies in Nevada

Income as a share of the FPL	Prior to January 2014		After January 2014	
	Pregnant	Childless, Non-Pregnant Adult	Pregnant	Childless, Non-Pregnant Adult
0-99	Pregnancy-Conditional Medicaid		Pregnancy-Conditional Medicaid	Medicaid
100-138	Pregnancy-Conditional Medicaid		Pregnancy-Conditional Medicaid	Medicaid
139-165	Pregnancy-Conditional Medicaid		Pregnancy-Conditional Medicaid	Marketplace
166-400			Marketplace	Marketplace

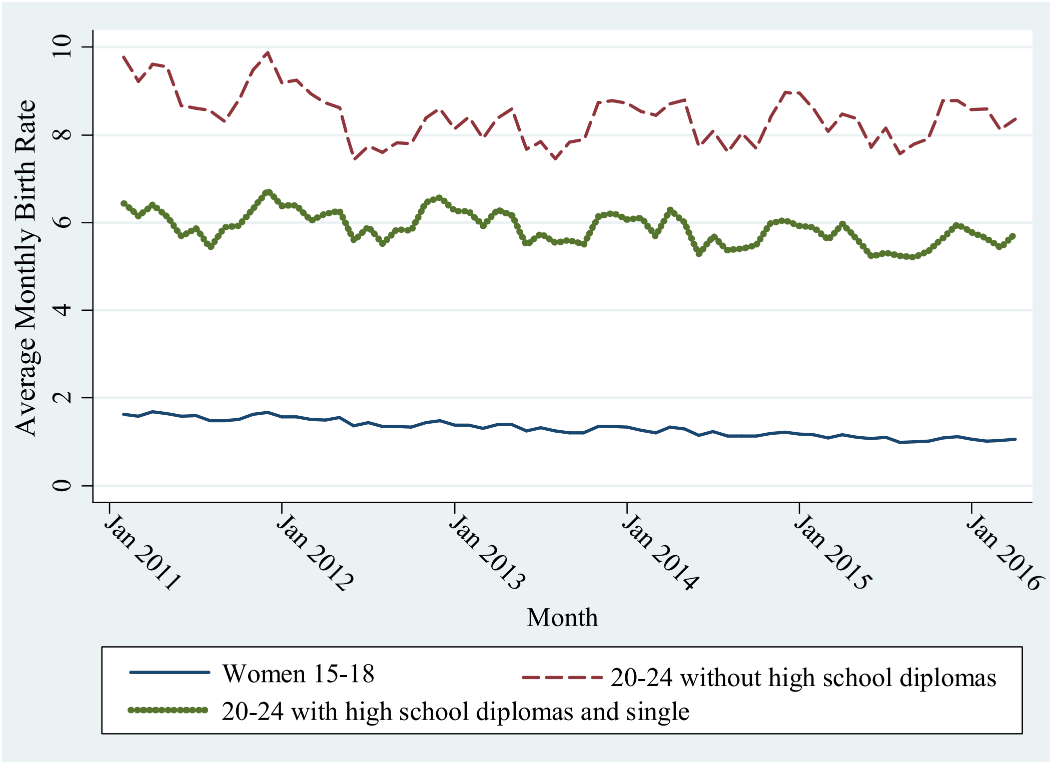
Table A.3 Detailed Eligibility Groups for Medicaid in Nevada

Income as a share of the FPL	Prior to January 2014		After January 2014	
	Pregnant	Childless, Non-Pregnant Adult	Pregnant	Childless, Non-Pregnant Adult
0-99	Medicaid Eligible when Pregnant, Ineligible Otherwise		Always Medicaid Eligible	
100-138	Medicaid Eligible when Pregnant, Ineligible Otherwise		Always Medicaid Eligible	
139-165	Medicaid Eligible when Pregnant, Ineligible Otherwise		Medicaid Eligible when Pregnant, Non-Medicaid Eligible Otherwise	
166-400			Always Eligible for Non-Medicaid Insurance	



Note: Eligibility for childless women 20-24 nationally.
Source: ACS data and policy information

Figure A.5 Changes in Detailed Eligibility Measures



Source: US natality records, the ACS, and the Census Bureau's "Estimates of the Population of States by Age, Sex, Race, and Hispanic Origin" data.

Figure A.6 Birth Trends for Teens and Similar 20-24 Year Old Groups

Table A.4 Event Study

	Ages 20-24			Ages 25-34			Ages 35-45		
	Medicaid Enrollment	Non-Medicaid Enrollment	ln(birthrate +1)	Medicaid Enrollment	Non-Medicaid Enrollment	ln(birthrate +1)	Medicaid Enrollment	Non-Medicaid Enrollment	ln(birthrate +1)
2011X Below 138%	0.0045	0.0189	0.101	-0.0087	-0.0858*	0.0374	-0.0307	0.0322	-0.0876
	(0.028)	(0.048)	(0.062)	(0.023)	(0.038)	(0.027)	(0.019)	(0.028)	(0.072)
2012 X Below 138%	0.0369	0.00174	-0.00821	0.00766	-0.0534	-0.0454	0.0176	0.00732	-0.0793
	(0.036)	(0.048)	(0.053)	(0.027)	(0.031)	(0.031)	(0.020)	(0.025)	(0.054)
2014 X Below 138%	0.0693*	0.214***	0.09	0.0184	0.00328	0.165**	0.0059	0.0840*	0.182**
	(0.029)	(0.047)	(0.047)	(0.021)	(0.038)	(0.054)	(0.017)	(0.034)	(0.064)
2015 X Below 138%	0.058	0.226***	0.197**	0.0590**	0.0547*	0.199**	0.0164	0.0446	0.228**
	(0.035)	(0.051)	(0.058)	(0.021)	(0.022)	(0.060)	(0.025)	(0.026)	(0.074)
2016 X Below 138%	0.0867*	0.332***	0.347***	0.0990***	0.0882*	0.254***	0.0822*	0.0784*	-0.0287
	(0.040)	(0.051)	(0.093)	(0.022)	(0.038)	(0.051)	(0.031)	(0.036)	(0.095)
2011 X Expander X Below 138%	-0.00307	-0.0489	-0.107	0.00423	0.0392	0.125*	0.00779	-0.00303	0.0692
	(0.039)	(0.055)	(0.073)	(0.034)	(0.045)	(0.055)	(0.026)	(0.037)	(0.087)
2012 X Expander X Below 138%	-0.0757	-0.00344	-0.0563	0.00225	-0.00428	0.0761	0.00124	-0.0143	-0.00148
	(0.045)	(0.061)	(0.061)	(0.033)	(0.041)	(0.042)	(0.034)	(0.038)	(0.071)
2014 X Expander X Below 138%	0.0503	-0.151**	-0.0203	0.132***	-0.0173	-0.0452	0.126***	-0.063	-0.232*
	(0.057)	(0.052)	(0.064)	(0.031)	(0.050)	(0.058)	(0.033)	(0.046)	(0.095)
2015 X Expander X Below 138%	0.154*	-0.0745	-0.0195	0.166***	-0.0125	-0.124*	0.219***	-0.0524	-0.203
	(0.071)	(0.063)	(0.085)	(0.040)	(0.034)	(0.059)	(0.051)	(0.043)	(0.103)
2016 X Expander X Below 138%	0.116	-0.175**	-0.185	0.136**	-0.0188	-0.0717	0.160**	-0.0267	-0.0544
	(0.066)	(0.060)	(0.148)	(0.041)	(0.042)	(0.072)	(0.056)	(0.052)	(0.105)
2011 X 139 to 400%	-0.0314	0.006	0.161*	-0.00639	0.0179	-0.0601	0.0159	-0.0196	-0.134*
	(0.042)	(0.057)	(0.061)	(0.017)	(0.023)	(0.031)	(0.019)	(0.030)	(0.063)
2012 X 139 to 400%	0.0259	0.00506	0.0168	-0.0132	0.0217	0.0111	-0.0252	-0.0135	-0.0692
	(0.030)	(0.048)	(0.084)	(0.017)	(0.022)	(0.031)	(0.022)	(0.038)	(0.061)
2014 X 139 to 400%	0.0898*	0.191**	0.132**	0.0308	0.0691**	0.0169	0.034	0.0227	0.0109
	(0.036)	(0.062)	(0.047)	(0.019)	(0.025)	(0.036)	(0.021)	(0.039)	(0.057)
2015 X 139 to 400%	0.154***	0.239***	0.201***	0.0438*	0.0987***	0.0456	0.0511*	0.0965**	-0.00881
	(0.039)	(0.066)	(0.057)	(0.017)	(0.026)	(0.044)	(0.024)	(0.034)	(0.048)
2016 X 139 to 400%	0.147***	0.305***	0.388***	0.0483*	0.108***	0.115*	0.0632**	0.0141	0.0664
	(0.041)	(0.069)	(0.098)	(0.020)	(0.025)	(0.044)	(0.023)	(0.037)	(0.086)
N=	6,230	6,230	7,147	6,676	6,676	7,325	6,591	6,591	7,237
P Value of Joint Test for Placebo Coefficients	0.078	0.967	0.021	0.901	0.160	0.008	0.254	0.826	0.020

Notes: Data covers insurance outcomes and births conceived from 2011-2016 for childless women 20-45. Regressions are run separately for each age group, so each panel represents the coefficients of interest from separate regressions. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. *$p < .05$, ** $p < .01$, *** $p < .001$

Table A.5 Placebo Test

	Medicaid Enrollment			Non-Medicaid Enrollment			ln(monthly birthrate +1/12)		
	1 year	2 year	3 year	1 year	2 year	3 year	12 months	18 month	24 months
Aged 20-24									
Medicaid Eligibility	0.0109 (0.021)	-0.00768 (0.013)	-0.00883 (0.011)	0.00343 (0.028)	0.0197 (0.019)	0.0405*** (0.010)	0.0317 (0.032)	0.0197 (0.034)	0.0278 (0.032)
NMSI Eligibility	-0.00328 (0.012)	-0.0003 (0.012)	0.00602 (0.011)	-0.0372 (0.022)	-0.0266 (0.020)	-0.0237 (0.020)	-0.0264 (0.027)	-0.0482 (0.024)	-0.0315 (0.022)
N=	4,544	4,544	4,544	4,544	4,544	4,544	51,540	51,540	51,540
Aged 25-34									
Medicaid Eligibility	0.00247 (0.029)	-0.0196 (0.023)	-0.0119 (0.018)	-0.0138 (0.040)	-0.0346 (0.033)	-0.00275 (0.025)	-0.0245 (0.040)	-0.0101 (0.040)	0.0102 (0.031)
NMSI Eligibility	-0.0236 (0.019)	-0.0259* (0.013)	-0.0272 (0.014)	-0.011 (0.022)	-0.011 (0.018)	0.000277 (0.015)	0.0595 (0.043)	0.0639 (0.042)	0.0804 (0.045)
N=	4,823	4,823	4,823	4,823	4,823	4,823	52,507	52,507	52,507
Aged 35-45									
Medicaid Eligibility	0.0049 (0.027)	0.00317 (0.022)	-0.00658 (0.018)	-0.0282 (0.046)	-0.0244 (0.035)	0.00207 (0.027)	-0.0149 (0.045)	0.0353 (0.048)	0.0527 (0.044)
NMSI Eligibility	-0.00306 (0.017)	-0.002 (0.011)	0.00525 (0.010)	0.0159 (0.022)	-0.0238 (0.017)	-0.0536** (0.019)	0.0113 (0.085)	0.0443 (0.079)	0.0907 (0.082)
N=	4,788	4,788	4,788	4,788	4,788	4,788	51,808	51,808	51,808

Notes: The insurance placebo tests are ran on annual data from 2008-2013 with policy variables are lagged by 1, 2, or 3 years. The birth rate placebo tests are ran on monthly data for conceptions from 2010-2013 with policy variables lagged by 12, 18, or 24 months. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. *$p < .05$, $^{**}p < .01$, $^{***}p < .001$. Of the 54 variables of interest, only 3 are significant correspond to 5.5%, right around the share expected by chance.

APPENDIX B. SUPPLEMENTAL MATERIAL FOR CHAPTER 2

Table B.1 Event Study of Pregnancy Outcomes

	Prenatal in 1st Trimester	Pre-Pregnancy Smoker	3rd Trimester Smoker	Inadequate Weight Gain	Adequate Weight Gain	Excessive Weight Gain	Gestational Diabetes	Gestational Hypertension
Non-Hispanic White								
2011X Below 138%	-0.00508	0.0238	0.0440**	-0.00393	0.0125	-0.00859	-0.0045	-0.00426
	-0.0108	-0.0132	-0.0126	-0.0121	-0.00766	-0.00961	-0.00665	-0.00676
2012 X Below 138%	(0.014)	0.015	0.017	(0.005)	0.009	(0.004)	(0.008)	(0.005)
	-0.0113	-0.0158	-0.011	-0.00986	-0.0131	-0.0163	-0.00672	-0.00668
2011 X Expander X Below 138%	0.021	0.017	(0.009)	0.009	-0.0339*	0.025	(0.000)	0.015
	(0.019)	(0.017)	(0.019)	(0.016)	(0.015)	(0.019)	(0.011)	(0.010)
2012 X Expander X Below 138%	0.030	0.023	0.013	(0.006)	(0.026)	0.032	0.017	0.010
	(0.015)	(0.021)	(0.017)	(0.016)	(0.018)	(0.020)	(0.010)	(0.010)
2011 X 139 to 400%	0.001	0.015	0.004	(0.007)	0.016	(0.008)	0.002	0.001
	(0.013)	(0.008)	(0.006)	(0.011)	(0.011)	(0.012)	(0.006)	(0.005)
2012 X 139 to 400%	(0.008)	0.002	(0.000)	(0.002)	0.006	(0.004)	(0.000)	0.005
	(0.008)	(0.007)	(0.006)	(0.011)	(0.009)	(0.009)	(0.005)	(0.006)
N=	1,264,319	1,269,126	1,268,987	1,259,021	1,259,021	1,259,021	1,300,943	1,300,943
Non-Hispanic Black								
2011X Below 138%	-0.0116	0.00735	0.0151	0.0121	-0.00171	-0.0104	0.00901	0.00336
	-0.0363	-0.0137	-0.00999	-0.0229	-0.0201	-0.0184	-0.0075	-0.0162
2012 X Below 138%	(0.003)	(0.004)	(0.009)	0.009	(0.031)	0.022	0.016	(0.007)
	-0.0297	-0.0101	-0.0088	-0.0206	-0.0246	-0.0167	-0.0106	-0.0104
2011 X Expander X Below 138%	0.050	0.0551**	0.020	(0.005)	(0.014)	0.019	0.015	0.015
	(0.038)	(0.020)	(0.018)	(0.031)	(0.032)	(0.034)	(0.015)	(0.023)
2012 X Expander X Below 138%	0.060	0.032	0.017	0.020	0.015	(0.035)	(0.004)	0.005
	(0.034)	(0.017)	(0.012)	(0.029)	(0.038)	(0.039)	(0.018)	(0.019)
2011 X 139 to 400%	0.014	-0.0340*	(0.013)	0.024	(0.007)	(0.017)	(0.003)	(0.010)
	(0.027)	(0.014)	(0.009)	(0.027)	(0.041)	(0.031)	(0.010)	(0.020)
2012 X 139 to 400%	0.033	(0.027)	(0.004)	0.0527*	0.006	(0.059)	0.007	0.004
	(0.028)	(0.014)	(0.009)	(0.023)	(0.029)	(0.037)	(0.012)	(0.018)
N=	389,288	397,834	397,780	388,591	388,591	388,591	409,176	409,176

Table B.1 Event Study of Pregnancy Outcomes Continued

	Prenatal in 1st Trimester	Pre-Pregnancy Smoker	3rd Trimester Smoker	Inadequate Weight Gain	Adequate Weight Gain	Excessive Weight Gain	Gestational Diabetes	Gestational Hypertension
Non-Hispanic Other								
2011X Below 138%	-0.0458	0.0298	0.0102	-0.0312	0.0456	-0.0143	-0.00741	0.0136
	-0.0387	-0.0223	-0.0162	-0.0322	-0.0316	-0.0257	-0.0125	-0.0122
2012 X Below 138%	0.000	0.034	0.014	0.004	0.033	(0.037)	0.028	0.013
	-0.0309	-0.0203	-0.0123	-0.026	-0.033	-0.0322	-0.0196	-0.0171
2011 X Expander X Below 138%	0.092	(0.022)	0.030	0.0949*	(0.089)	(0.006)	0.024	(0.024)
	(0.062)	(0.031)	(0.025)	(0.044)	(0.049)	(0.043)	(0.020)	(0.020)
2012 X Expander X Below 138%	0.063	(0.001)	0.020	0.021	(0.025)	0.004	(0.025)	(0.028)
	(0.050)	(0.035)	(0.021)	(0.040)	(0.048)	(0.045)	(0.029)	(0.024)
2011 X 139 to 400%	0.043	0.007	(0.008)	0.012	(0.028)	0.016	0.013	0.009
	(0.047)	(0.018)	(0.012)	(0.031)	(0.034)	(0.032)	(0.013)	(0.020)
2012 X 139 to 400%	0.028	(0.014)	(0.010)	0.025	(0.042)	0.017	0.013	0.017
	(0.029)	(0.018)	(0.011)	(0.026)	(0.030)	(0.033)	(0.019)	(0.015)
N=	119,747	121,301	121,264	119,795	119,795	119,795	124,531	124,531
Hispanic								
2011X Below 138%	0.00103	0.00656	0.00386	0.0241	0.0155	-0.0396	0.00696	0.00861
	-0.0148	-0.00929	-0.00494	-0.0182	-0.0252	-0.0263	-0.0119	-0.00891
2012 X Below 138%	0.007	0.016	0.008	0.014	(0.001)	(0.013)	0.016	0.000
	-0.0176	-0.00814	-0.00599	-0.0159	-0.0382	-0.0399	-0.0142	-0.00905
2011 X Expander X Below 138%	0.019	-0.0357*	(0.014)	(0.009)	(0.018)	0.027	(0.003)	(0.008)
	(0.021)	(0.014)	(0.008)	(0.025)	(0.034)	(0.035)	(0.015)	(0.018)
2012 X Expander X Below 138%	0.042	-0.0562***	(0.017)	(0.034)	0.038	(0.004)	(0.025)	0.008
	(0.030)	(0.015)	(0.009)	(0.024)	(0.042)	(0.044)	(0.017)	(0.016)
2011 X 139 to 400%	0.001	0.009	0.001	(0.017)	0.031	(0.014)	0.009	0.005
	(0.028)	(0.008)	(0.006)	(0.021)	(0.041)	(0.034)	(0.009)	(0.010)
2012 X 139 to 400%	0.011	0.009	0.002	(0.002)	0.028	(0.026)	0.013	0.008
	(0.019)	(0.010)	(0.005)	(0.016)	(0.027)	(0.029)	(0.010)	(0.010)
N=	415,763	429,221	429,243	418,777	418,777	418,777	432,943	432,943

Notes: Sample consists of childless women aged 20-45. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. **p* < .05, ** *p* < .01, *** *p* < .001

Table B.2 Event Study of Delivery Outcomes

	Medicaid Paid Birth	Anesthesia	Cesarean Section	LBWT	Preterm	NICU	Breastfeeding
Non-Hispanic White							
2011X Below 138%	0.0524**	-0.0272*	-0.0173	0.00037	0.00367	-0.00267	-0.0619***
	-0.0158	-0.0112	-0.0169	-0.00518	-0.00506	-0.00683	-0.0105
2012 X Below 138%	0.022	0.003	(0.005)	(0.009)	(0.007)	(0.006)	-0.0341**
	-0.0141	-0.01	-0.0119	-0.00693	-0.00627	-0.00851	-0.01
2011 X Expander X Below 138%	0.014	0.0440*	0.016	(0.009)	(0.006)	(0.005)	0.002
	(0.039)	(0.019)	(0.024)	(0.011)	(0.013)	(0.011)	(0.021)
2012 X Expander X Below 138%	0.009	(0.002)	0.002	(0.001)	0.001	(0.003)	0.020
	(0.024)	(0.016)	(0.019)	(0.010)	(0.010)	(0.010)	(0.020)
2011 X 139 to 400%	0.007	0.002	(0.014)	(0.002)	0.008	0.004	(0.012)
	(0.013)	(0.013)	(0.008)	(0.005)	(0.010)	(0.008)	(0.011)
2012 X 139 to 400%	0.012	(0.006)	-0.0227*	(0.001)	(0.004)	0.004	(0.011)
	(0.011)	(0.010)	(0.010)	(0.006)	(0.008)	(0.007)	(0.008)
N=	1,292,334	1,301,785	1,301,946	1,302,574	1,202,551	1,299,815	1,283,808
Non-Hispanic Black							
2011X Below 138%	0.00468	0.00837	-0.0177	0.00704	0.0178	0.0107	-0.0529**
	-0.0269	-0.019	-0.0347	-0.0194	-0.0162	-0.0225	-0.0181
2012 X Below 138%	0.014	0.018	(0.023)	0.004	(0.012)	(0.015)	(0.031)
	-0.0224	-0.0168	-0.0282	-0.0177	-0.019	-0.0128	-0.0181
2011 X Expander X Below 138%	0.027	(0.000)	0.033	(0.010)	(0.001)	(0.010)	0.005
	(0.053)	(0.033)	(0.039)	(0.029)	(0.023)	(0.028)	(0.038)
2012 X Expander X Below 138%	(0.020)	(0.034)	0.011	(0.034)	(0.007)	(0.013)	(0.049)
	(0.035)	(0.026)	(0.033)	(0.019)	(0.026)	(0.027)	(0.038)
2011 X 139 to 400%	0.034	(0.000)	(0.021)	0.016	(0.011)	(0.004)	(0.026)
	(0.035)	(0.026)	(0.024)	(0.016)	(0.018)	(0.016)	(0.025)
2012 X 139 to 400%	(0.025)	0.001	(0.030)	0.002	(0.029)	(0.008)	0.015
	(0.020)	(0.028)	(0.024)	(0.015)	(0.022)	(0.013)	(0.025)
N=	407,053	409,640	409,751	409,946	371,947	408,932	402,138

Table B.2 Event Study of Delivery Outcomes Continued

	Medicaid Paid Birth	Anesthesia	Cesarean Section	LBWT	Preterm	NICU	Breastfeeding
Non-Hispanic Other							
2011X Below 138%	0.0914**	0.00266	0.028	0.0363	-0.00901	0.0163	-0.0968**
	-0.0298	-0.0363	-0.033	-0.0195	-0.0215	-0.0173	-0.0307
2012 X Below 138%	0.046	(0.002)	0.024	0.023	(0.040)	0.011	(0.045)
	-0.0309	-0.044	-0.0267	-0.0177	-0.0223	-0.0271	-0.0231
2011 X Expander X Below 138%	(0.115)	(0.052)	(0.012)	-0.0704*	(0.012)	(0.036)	0.005
	(0.063)	(0.078)	(0.043)	(0.028)	(0.040)	(0.026)	(0.042)
2012 X Expander X Below 138%	(0.073)	(0.033)	(0.012)	(0.032)	0.028	(0.057)	(0.012)
	(0.064)	(0.077)	(0.038)	(0.022)	(0.033)	(0.031)	(0.042)
2011 X 139 to 400%	(0.013)	0.036	0.037	0.025	0.0588*	0.025	0.008
	(0.040)	(0.039)	(0.028)	(0.018)	(0.027)	(0.020)	(0.028)
2012 X 139 to 400%	0.023	0.018	0.031	0.015	0.003	0.025	(0.002)
	(0.036)	(0.039)	(0.029)	(0.012)	(0.025)	(0.021)	(0.024)
N=	123,463	124,576	124,629	124,668	113,608	124,372	122,665
Hispanic							
2011X Below 138%	-0.0498*	0.0134	-0.0613*	0.0161*	-0.0281*	-0.0072	-0.0271
	-0.0221	-0.0143	-0.0243	-0.0067	-0.0128	-0.00819	-0.0162
2012 X Below 138%	(0.041)	0.0532*	-0.0360*	0.011	(0.020)	(0.003)	(0.020)
	-0.0216	-0.0232	-0.0169	-0.00987	-0.0115	-0.0121	-0.0129
2011 X Expander X Below 138%	0.0820*	(0.025)	0.0790**	(0.032)	0.015	0.017	0.029
	(0.032)	(0.029)	(0.027)	(0.017)	(0.021)	(0.015)	(0.029)
2012 X Expander X Below 138%	0.036	(0.041)	0.017	(0.011)	(0.007)	0.002	0.009
	(0.031)	(0.027)	(0.031)	(0.013)	(0.020)	(0.014)	(0.021)
2011 X 139 to 400%	0.010	(0.022)	(0.026)	0.0234*	0.004	0.003	(0.005)
	(0.040)	(0.029)	(0.025)	(0.010)	(0.022)	(0.019)	(0.014)
2012 X 139 to 400%	(0.000)	0.014	(0.018)	0.002	(0.002)	(0.012)	(0.015)
	(0.020)	(0.028)	(0.020)	(0.012)	(0.017)	(0.017)	(0.015)
N=	428,678	433,157	433,172	433,313	397,527	432,796	429,454

Notes: Sample consists of childless women aged 20-45. All regressions contain the unemployment rate, state-by-group fixed effects, and month-year fixed effects. The regressions are weighed by state-by-group population and clustered by state. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

B.1 Placebo Tests Robustness Checks

This section presents the placebo tests with policy variables are lagged by 6, 12, or 18 months for pre-period data. Here the key lagged coefficients, which ideally should be insignificant, are reported. The sample consists of women aged 20-45 giving birth for the first time. All regressions contain state-by-group fixed effects and month-year fixed effects. *$p < .05$, ** $p < .01$, *** $p < .001$.

Table B.3 Placebo Test--First Set of Outcomes

	Medicaid Paid Birth			Prenatal in 1st Trimester			Pre-Pregnancy Smoker			3rd Trimester Smoker		
	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months
Non-Hispanic White												
Medicaid	-0.0427*	-0.0302**	-0.0245*	0.0044	0.0143	-0.0125	0.0004	0.0147	0.0006	0.0091	-0.0092	0.0041
Eligibility	(0.019)	(0.011)	(0.011)	(0.012)	(0.014)	(0.008)	(0.014)	(0.010)	(0.008)	(0.007)	(0.010)	(0.008)
NMSI	-0.0145*	-0.0039	-0.0069	-0.0198**	-0.0126**	-0.0203***	-0.0018	-0.0037	-0.0118**	0.0093	0.0042	0.0041
Eligibility	(0.007)	(0.005)	(0.004)	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)
N=	1,716,604	1,716,604	1,716,604	1,700,521	1,700,521	1,700,521	1,700,373	1,700,373	1,700,373	1,707,094	1,707,094	1,707,094
Non-Hispanic Black												
Medicaid	-0.0542*	-0.0397***	-0.0114	-0.0110	-0.0020	0.0049	-0.0081	-0.0040	0.0099	0.0331*	0.0184	0.0187
Eligibility	(0.025)	(0.008)	(0.012)	(0.014)	(0.011)	(0.008)	(0.013)	(0.012)	(0.007)	(0.015)	(0.013)	(0.010)
NMSI	-0.0101	0.0028	0.0040	-0.0090	0.0014	-0.0080	-0.0047	-0.0076	-0.0075	0.0112	-0.0026	-0.0008
Eligibility	(0.019)	(0.018)	(0.015)	(0.013)	(0.009)	(0.011)	(0.009)	(0.006)	(0.008)	(0.010)	(0.012)	(0.012)
N=	339,034	338,867	339,034	342,821	342,648	342,821	342,781	342,608	342,781	339,155	338,988	339,155
Non-Hispanic Other												
Medicaid	-0.0942*	-0.0651	-0.0336	-0.0063	0.0051	-0.0085	-0.0171*	-0.0044	-0.0096	0.0466	0.0012	0.0001
Eligibility	(0.038)	(0.036)	(0.020)	(0.011)	(0.012)	(0.009)	(0.007)	(0.008)	(0.006)	(0.030)	(0.028)	(0.019)
NMSI	-0.0263	-0.0003	0.0018	0.0018	-0.0122	-0.0123	0.0020	-0.0091	-0.0032	-0.0118	0.0148	0.0047
Eligibility	(0.019)	(0.017)	(0.012)	(0.009)	(0.009)	(0.009)	(0.005)	(0.005)	(0.004)	(0.020)	(0.018)	(0.018)
N=	185,202	185,051	185,202	187,091	186,929	187,091	187,056	186,894	187,056	186,009	185,859	186,009
Hispanic												
Medicaid	-0.0473*	-0.0459**	-0.0199	0.0086	0.0083	-0.0016	-0.0005	-0.0062	-0.0029	0.0041	0.0086	-0.0124
Eligibility	(0.018)	(0.014)	(0.018)	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.026)	(0.018)	(0.021)
NMSI	-0.0185	-0.0092	-0.0019	-0.0068	-0.0099	-0.0071	-0.0037	-0.0016	-0.0009	0.0051	0.0042	0.0055
Eligibility	(0.012)	(0.009)	(0.012)	(0.004)	(0.006)	(0.005)	(0.004)	(0.003)	(0.003)	(0.010)	(0.007)	(0.009)
N=	390,923	390,794	390,923	403,708	403,578	403,708	403,742	403,612	403,742	394,897	394,770	394,897

Table B.4 Placebo Test--Second Set of Outcomes

	Adequate Weight Gain			Excessive Weight Gain			Gestational Diabetes			Gestational Hypertension		
	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months
Non-Hispanic White												
Medicaid	0.0278**	0.0165**	0.0140*	-0.0369**	-0.0072	-0.0181*	0.0091	0.0029	-0.0030	-0.0074	-0.0004	-0.0012
Eligibility	(0.008)	(0.005)	(0.006)	(0.012)	(0.008)	(0.008)	(0.007)	(0.005)	(0.004)	(0.011)	(0.005)	(0.005)
NMSI	0.0009	0.0009	0.0042	-0.0102	-0.0051	-0.0082	-0.0049	-0.0033	-0.0026	-0.0021	0.0003	-0.0007
Eligibility	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
N=	1,707,094	1,707,094	1,707,094	1,707,094	1,707,094	1,707,094	1,768,968	1,768,968	1,768,968	1,768,968	1,768,968	1,768,968
Non-Hispanic Black												
Medicaid	-0.0178	-0.0090	-0.0083	-0.0152	-0.0095	-0.0105	0.0006	-0.0125	-0.0095	0.0157	0.0020	-0.0076
Eligibility	(0.016)	(0.019)	(0.011)	(0.019)	(0.018)	(0.015)	(0.008)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)
NMSI	0.0038	0.0038	-0.0043	-0.0150	-0.0011	0.0050	-0.0073	-0.0069	-0.0123*	-0.0041	-0.0149	-0.0202*
Eligibility	(0.015)	(0.012)	(0.015)	(0.020)	(0.014)	(0.019)	(0.008)	(0.006)	(0.005)	(0.010)	(0.013)	(0.010)
N=	339,155	338,988	339,155	339,155	338,988	339,155	358,263	358,088	358,263	358,263	358,088	358,263
Non-Hispanic Other												
Medicaid	0.0117	0.0535*	0.0222	-0.0582	-0.0547*	-0.0223	-0.0186	-0.0162	-0.0148	0.0104	0.0219	0.0058
Eligibility	(0.026)	(0.023)	(0.026)	(0.038)	(0.027)	(0.025)	(0.015)	(0.017)	(0.014)	(0.009)	(0.011)	(0.009)
NMSI	0.0002	-0.0101	-0.0143	0.0116	-0.0047	0.0096	-0.0081	-0.0129	-0.0191*	-0.0119	-0.0046	-0.0065
Eligibility	(0.018)	(0.014)	(0.011)	(0.021)	(0.020)	(0.018)	(0.015)	(0.008)	(0.007)	(0.010)	(0.009)	(0.008)
N=	186,009	185,859	186,009	186,009	185,859	186,009	193,954	193,791	193,954	193,954	193,791	193,954
Hispanic												
Medicaid	-0.0038	-0.0075	0.0224	-0.0003	-0.0011	-0.0100	0.0128	0.0145	0.0191**	-0.0099	0.0034	0.0068
Eligibility	(0.021)	(0.020)	(0.017)	(0.018)	(0.019)	(0.012)	(0.007)	(0.008)	(0.006)	(0.007)	(0.006)	(0.005)
NMSI	-0.0092	-0.0100	0.0081	0.0041	0.0058	-0.0136	-0.0086	-0.0036	0.0003	-0.0089	-0.0018	-0.0054
Eligibility	(0.017)	(0.015)	(0.011)	(0.013)	(0.011)	(0.012)	(0.008)	(0.005)	(0.006)	(0.007)	(0.005)	(0.004)
N=	394,897	394,770	394,897	394,897	394,770	394,897	409,139	409,009	409,139	409,139	409,009	409,139

Table B.5 Placebo Test--Third Set of Outcomes

	Medicaid Paid Birth			Anesthesia			Cesarean Section			LBWT		
	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months
Non-Hispanic White												
Medicaid	-0.0628	-0.0501	-0.0695	0.0037	0.0065	0.0023	-0.0038	-0.0031	-0.0047	-0.0012	-0.007	-0.0141
Eligibility	-0.04	-0.036	-0.044	-0.005	-0.006	-0.005	-0.009	-0.005	-0.004	-0.009	-0.007	-0.007
NMSI	-0.0105	-0.0001	-0.0025	0.0064	0.00684*	0.00802*	0.005	0.0031	0.0048	0.0059	0.0044	0.0061
Eligibility	-0.013	-0.01	-0.01	-0.004	-0.003	-0.004	-0.004	-0.003	-0.003	-0.004	-0.004	-0.004
N=	1,756,791	1,756,791	1,756,791	1,771,585	1,771,585	1,771,585	1,643,151	1,643,151	1,643,151	1,767,303	1,767,303	1,767,303
Non-Hispanic Black												
Medicaid	-0.023	-0.0316	-0.0216	0.0056	0.0026	0.001	0.033	0.0141	0.0094	0.0081	-0.0046	-0.0160*
Eligibility	-0.033	-0.044	-0.056	-0.013	-0.01	-0.01	-0.02	-0.01	-0.009	-0.015	-0.012	-0.008
NMSI	-0.0167	-0.0074	-0.0246	0.0228*	0.0071	0.0026	0.0325**	0.0171	-0.0022	0.0145	0.0012	-0.0077
Eligibility	-0.028	-0.028	-0.031	-0.009	-0.008	-0.008	-0.01	-0.01	-0.011	-0.012	-0.011	-0.011
N=	355,849	355,678	355,849	359,152	358,977	359,152	326,797	326,635	326,797	358,099	357,925	358,099
Non-Hispanic Other												
Medicaid	-0.0082	-0.0574	-0.0791	0.0035	0.0171	0.0032	0.0088	-0.003	-0.0052	-0.0344*	-0.0281	-0.0307*
Eligibility	-0.046	-0.066	-0.055	-0.015	-0.011	-0.012	-0.02	-0.027	-0.017	-0.015	-0.021	-0.015
NMSI	-0.0054	0.0275	0.0432*	0.0038	0.0015	0.0012	0.0052	-0.0096	-0.0002	0.0147	0.0128	0.021
Eligibility	-0.017	-0.016	-0.017	-0.013	-0.011	-0.007	-0.015	-0.013	-0.013	-0.008	-0.016	-0.014
N=	192,191	192,034	192,191	194,217	194,054	194,217	178,540	178,388	178,540	193,757	193,595	193,757
Hispanic												
Medicaid	-0.0042	-0.0306	-0.0184	-0.008	0.004	-0.0024	-0.0099	-0.0054	-0.0181	-0.0022	0.0087	-0.0028
Eligibility	-0.051	-0.039	-0.043	-0.009	-0.009	-0.009	-0.009	-0.012	-0.011	-0.009	-0.007	-0.007
NMSI	-0.0292	-0.0097	-0.0241	-0.0011	-0.0097	-0.0074	-0.0005	-0.0085	-0.0161*	0.0053	-0.0025	-0.0104
Eligibility	-0.025	-0.024	-0.025	-0.006	-0.006	-0.005	-0.008	-0.01	-0.008	-0.005	-0.009	-0.009
N=	403,743	403,614	403,743	409,580	409,450	409,580	376,152	376,031	376,152	409,022	408,894	409,022

Table B.6 Placebo Test--Fourth Set of Outcomes

	Preterm			NICU			Breastfeeding		
	6 months	12 months	18 months	6 months	12 months	18 months	6 months	12 months	18 months
Non-Hispanic White									
Medicaid	-0.0203	-0.0098	0.0193	-0.0169	-0.0165	-0.0049	0.0026	0.0041	-0.0045
Eligibility	-0.01	-0.01	-0.011	-0.019	-0.026	-0.03	-0.016	-0.013	-0.012
NMSI Eligibility	0.0235**	0.0240***	0.0272***	0.0297	0.0286	0.022	0.0072	0.0088	0.0055
	-0.007	-0.006	-0.006	-0.017	-0.015	-0.014	-0.007	-0.005	-0.005
N=	1,745,341	1,745,341	1,745,341	1,770,091	1,770,091	1,770,091	1,770,662	1,770,662	1,770,662
Non-Hispanic Black									
Medicaid	-0.0159	0.0014	0.022	0.0249	0.0055	0.0124	-0.0093	-0.0002	0.0065
Eligibility	-0.028	-0.018	-0.019	-0.034	-0.028	-0.03	-0.013	-0.015	-0.018
NMSI Eligibility	-0.0221	-0.0048	0.009	0.042	0.0409*	0.031	-0.0092	-0.006	-0.0155
	-0.019	-0.013	-0.017	-0.022	-0.018	-0.018	-0.016	-0.014	-0.014
N=	351,861	351,688	351,861	358,828	358,654	358,828	358,961	358,786	358,961
Non-Hispanic Other									
Medicaid	-0.0339	-0.0197	0.0086	-0.0730*	-0.0316	0.0022	-0.0376	-0.0334	-0.0452*
Eligibility	-0.027	-0.044	-0.027	-0.029	-0.037	-0.04	-0.022	-0.021	-0.018
NMSI Eligibility	-0.0044	0.019	0.0135	0.0639*	0.0324	0.0116	0.0257	-0.0055	0.0004
	-0.018	-0.016	-0.017	-0.031	-0.029	-0.025	-0.023	-0.023	-0.018
N=	191,610	191,455	191,610	194,055	193,892	194,055	194,137	193,974	194,137
Hispanic									
Medicaid	-0.0297	-0.0316*	-0.0035	0.0517	0.0481*	0.0302	0.0438**	0.0217	0.018
Eligibility	-0.017	-0.014	-0.024	-0.027	-0.023	-0.033	-0.016	-0.011	-0.011
NMSI Eligibility	0.0086	0.0214**	0.0234**	0.0231	0.0073	-0.0112	0.0300**	0.0155*	0.0166
	-0.01	-0.008	-0.008	-0.019	-0.023	-0.027	-0.01	-0.008	-0.009
N=	405,956	405,829	405,956	409,386	409,256	409,386	409,464	409,334	409,464

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VITA

Makayla Palmer grew up outside of Boulder, Colorado. For her undergraduate studies, she attended Whitworth University in Spokane, Washington. She did a Research Experience for Undergraduates internship with the Andrew Young School at Georgia State University the summer of 2012. In May 2013, she graduated from Whitworth with Bachelor of Arts degrees in Mathematical Economics, Mathematics, and Spanish as well as minors in Business and Peace Studies.

She began her doctoral studies at Georgia State University in the fall of 2013. She worked as a research assistant for Chuck Courtemanche and then Jim Marton. During the 2015-2016 academic year she taught Principles of Macroeconomics. Starting summer 2016 she also worked as a research assistant for Stephen Kay at the Federal Reserve Bank of Atlanta. In December 2016 she received her master's degree in economics from Georgia State University.

Makayla received the Andrew Young Fellowship, a three-year fellowship for incoming doctoral students. She also received Ramsey Health Economics Summer Research grants for her work on foster care. She was awarded Outstanding Graduate Research Assistant twice and received the George Malanos Economics scholarship.

During the final year of the doctoral program, Makayla presented at the Southeastern Health Economics Study Group, Association for Public Policy Analysis and Management Fall Research Conference, the Southern Economic Association Conference, and the Annual Conference of the American Society of Health Economists.

Makayla has accepted a position as an assistant professor of economics at the University of Nevada, Las Vegas. She will be part of the new, interdisciplinary research group Health for

Nevada Initiative and continue to research health economics issues which focus on low-income and high cost populations.